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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCVII.

PART I.

LONDON,

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MDCCCVII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them, without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

The President and Council of the Royal Society adjudged the Medal on Sir Godfrey Copley's Donation for the year 1806, to Thomas Andrew Knight, Esq. F. R. S. for his various papers on Vegetation printed in the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *The Bakerian Lecture, on some chemical Agencies of Electricity.* *By* Humphry Davy, *Esq. F. R. S. M. R. I. A.*

Read November 20, 1806.

1. *Introduction.*

The chemical effects produced by electricity have been for some time objects of philosophical attention; but the novelty of the phenomena, their want of analogy to known facts, and the apparent discordance of some of the results, have involved the enquiry in much obscurity.

An attempt to elucidate the subject will not, I hope, be considered by the Society as unfitted to the design of the Bakerian Lecture. I shall have to detail some minute (and I fear tedious) experiments; but they were absolutely essential to the investigation. I shall likewise, however, be able to offer some illustrations of appearances which hitherto have not been fully explained, and to point out some new properties of one of the most powerful and general of material agents.

II. *On the Changes produced by Electricity in Water.*

The appearance of acid and alkaline matter in water acted on by a current of electricity, at the opposite electrified metallic surfaces was observed in the first chemical experiments made with the column of VOLTA.*

Mr. CRUICKSHANK† supposed that the acid was the nitrous acid and the alkali ammonia. M. DESORMES‡ soon after attempted to show by experiments, that muriatic acid and ammonia were the products, and M. BRUGNATELLI§ asserted the formation of a new and peculiar substance, which he has thought proper to call the electric acid. The experiments said to be made in Italy, and in this country, on the production of muriate of soda are recent,‖ and the discussions with regard to them still alive. As early as 1800, I had found that when separate portions of distilled water, filling two glass tubes connected by moist bladders, or any moist animal or vegetable substances, were submitted to the electrical action of the pile of VOLTA by means of gold wires, a nitro-muriatic solution of gold appeared in the tube containing the positive wire, or the wire transmitting the electricity, and a solution of soda in the opposite tube;¶ but I soon ascertained that the muriatic acid owed its appearance to the animal or vegetable matters employed; for when the same fibres of cotton were made use of in successive experiments, and washed after every process in a weak solution of nitric acid, the water in the apparatus

* NICHOLSON's Journal, 4to. Vol. IV. p. 183. † Ibid. Vol. IV. p. 261.

‡ *Annales de Chimie*, Tom. XXXVII. p. 233. § Phil. Mag. Vol. IX. p. 181.

‖ By M. PACCHIONI, and by Mr. PEELE. Phil. Mag. Vol. XXI. p. 279.

¶ I shewed the results of the experiment to Dr. BEDDOES at this time; and mentioned the circumstance to Sir JAMES HALL, Mr. CLAYFIELD, and other friends in 1801.

containing them, though acted on for a great length of time with a very strong power, at last produced no effect upon solution of nitrate of silver.

In cases when I had procured much soda, the glass at its point of contact with the wire seemed considerably corroded; and I was confirmed in my idea of referring the production of the alkali principally to this source, by finding that no fixed saline matter could be obtained, by electrifying distilled water in a single agate cup from two points of platina connected with the VOLTAIC battery. Similar conclusions with regard to the appearance of the muriatic acid had been formed by the GALVANIC Society of Paris, by Dr. WOLLASTON, who hit upon the happy expedient of connecting the tubes together by well-washed asbestus; and by M. M. BIOT and THENARD.*

Mr. SYLVESTER, however, in a Paper published in Mr. NICHOLSON'S Journal for last August, states, that though no fixed alkali or muriatic acid appears when a single vessel is employed; yet that they are both formed when two vessels are used. And to do away all objections with regard to vegetable substances or glass, he conducted his process in a vessel made of baked tobacco-pipe clay inserted in a crucible of platina. I have no doubt of the correctness of his results: but the conclusion appears objectionable. He conceives that he obtained fixed alkali, because the fluid after being heated and evaporated left a matter that tinged turmeric brown, which would have happened had it been lime, a substance that exists in considerable quantities in all pipe-clay; and even allowing the presence of fixed alkali, the materials employed for the manufacture of tobacco-pipes are not at all such as to exclude the combinations of this substance.

* No. XL. *Du Moniteur*, 1806.

I resumed the enquiry; I procured small cylindrical cups of agate, of the capacity of about $\frac{1}{4}$ of a cubic inch each. They were boiled for some hours in distilled water, and a piece of very white and transparent amianthus that had been treated in the same way was made to connect them together; they were filled with distilled water, and exposed by means of two platina wires to a current of electricity, from 150 pairs of plates of copper and zinc 4 inches square, made active by means of solution of alum. After 48 hours the process was examined: paper tinged with litmus plunged into the tube containing the transmitting or positive wire was immediately strongly reddened. Paper coloured by turmeric introduced into the other tube had its colour much deepened; the acid matter gave a very slight degree of turbidness to solution of nitrate of silver. The fluid that affected turmeric retained this property after being strongly boiled; and it appeared more vivid as the quantity became reduced by evaporation: carbonate of ammonia was mixed with it, and the whole dried and exposed to a strong heat: a minute quantity of white matter remained, which, as far as my examination could go, had the properties of carbonate of soda. I compared it with similar minute portions of the pure carbonates of potash and soda. It was not so deliquescent as the former of these bodies, and it formed a salt with nitric acid, which like nitrate of soda soon attracted moisture from a damp atmosphere, and became fluid.

This result was unexpected, but it was far from convincing me that the substances which I had obtained were *generated*. In a similar process, with glass tubes, carried on exactly under the same circumstances, and for the same time, I obtained a quantity of alkali which must have been more than twenty

times greater, but no traces of muriatic acid. There was much probability that the agate might contain some minute portion of saline matter, not easily detected by chemical analysis, either in combination, or intimate adhesion in its pores. To determine this, I repeated the experiment a second, a third, and a fourth time. In the second experiment turbidness was still produced by solution of nitrate of silver in the tube containing the acid, but it was less distinct; in the third process it was barely perceptible: and in the fourth the two fluids remained perfectly clear after the mixture. The quantity of alkaline matter diminished in every operation; and in the last process, though the battery had been kept in great activity for three days, the fluid possessed in a very slight degree only the power of acting on paper tinged with turmeric; but its alkaline property was very sensible to litmus paper slightly reddened, which is a much more delicate test: and after evaporation and the process by carbonate of ammonia, a barely perceptible quantity of fixed alkali was still left. The acid matter in the other tube was abundant; its taste was sour; it smelt like water over which large quantities of nitrous gas have been long kept; it did not affect solution of muriate of barytes; and a drop of it placed upon a polished plate of silver left after evaporation a black stain, precisely similar to that produced by extremely diluted nitrous acid.

After these results, I could no longer doubt that some saline matter existing in the agate tubes had been the source of the acid matter capable of precipitating nitrate of silver, and of much of the alkali. Four additional repetitions of the process, however, convinced me that there was likewise some other cause for the presence of this last substance; for it

continued to appear to the last, in quantities sufficiently distinguishable, and apparently equal in every case. I had used every precaution; I had included the tubes in glass vessels out of the reach of the circulating air; all the acting materials had been repeatedly washed with distilled water; and no part of them in contact with the fluid had been touched by the fingers.

The only substance which I could now conceive capable of furnishing the fixed alkali was the *water itself.* This water appeared pure by the tests of nitrate of silver and muriate of barytes; but potash and soda, as is well known, rise in small quantities in rapid distillations; and the New River water, which I made use of, contains animal and vegetable impurities, which it was easy to conceive might furnish neutral salts capable of being carried over in vivid ebullition.

To make the experiment in as refined a form as possible, I procured two hollow cones of pure gold containing about 25 grains of water each, they were filled with distilled water, connected together by a moistened piece of amianthus which had been used in the former experiments, and exposed to the action of a VOLTAIC battery of 100 pairs of plates of copper and zinc of 6 inches square, in which the fluid was a solution of alum and diluted sulphuric acid. In ten minutes the water in the negative tube had gained the power of giving a slight blue tint to litmus paper: and the water in the positive tube rendered it red. The process was continued for 14 hours; the acid increased in quantity during the whole time, and the water became at last very sour to the taste. The alkaline properties of the fluid in the other tube, on the contrary, remained stationary, and at the end of the time, it did not act upon litmus or turmeric paper more than in the first trial: the effect was less vivid after it had been strongly heated for a

minute; but evaporation and the usual process proved that *some* fixed alkali was present. The acid, as far as its properties were examined, agreed with pure nitrous acid having an excess of nitrous gas.

I *repeated* the experiment, and carried on the process for three days; at the end of which time the water in the tube was decomposed and evaporated to more than one half of its original quantity; the acid was strong, but the alkali in as minute a portion as in the last experiment. It acted indeed rather more vividly on the tests, on account of the greater diminution of the fluid, but presented the same results after being heated.

It was now impossible to doubt that the water contained some substance in very minute quantities, capable of causing the appearance of fixed alkali, but which was soon exhausted; and the question that immediately presented itself was, Is this substance saline matter carried over in distillation? or is it nitrogen gas which exists in minute portions in all water that has been exposed to air, and which, if an element of the fixed alkali, would under the circumstance of the experiment have been soon exhausted, whilst its absorption from the atmosphere would be impeded by the saturation of the water with hydrogen?

I was much more inclined to the former than to the latter supposition. I evaporated a quart of the distilled water that I had used, very slowly at a heat below 140° FAHRENHEIT, in a silver still; a solid matter remained, equal to $\frac{7}{10}$ of a grain; this matter had a saline but metallic taste, and was deliquescent when exposed to air: I could not obtain from it regular crystals; it did not affect turmeric or litmus, but a part

of it, after being heated red, in a silver crucible, exhibited strong alkaline properties. It was not possible to make a minute analysis of so small a quantity, but it appeared to me to be principally a mixture of nitrate of soda and nitrate of lead; and the metallic substance, it is most likely, was furnished by the condensing tube of the common still.

The existence of saline matter in the distilled water being thus distinct, it was easy to determine its operation in the experiment. I filled the two gold cones with water in the usual manner; that negatively electrified, soon attained the maximum of its effect upon turmeric paper. I then introduced into it a very minute portion of the substance obtained by the process of evaporation that has been just described; in less than two minutes its effects were evident; and in five minutes the tint of the paper was changed to a bright brown.

I now conceived that by collecting the water obtained in the second process of slow distillation I should be able to carry on the experiment without *any* appearance of fixed alkali, and the trial proved that I was not mistaken.

Some of this water was introduced into the gold tubes, and the amianthus moistened by it.

After two hours the water in the negative tube produced no effect upon turmeric paper; it did produce an effect upon litmus, which it required great minuteness of observation to perceive; but it wholly lost the power by being heated strongly for two or three minutes, so there is every reason for supposing that it was owing to a small quantity of ammonia.

I made a similar experiment with a portion of the same water in the tubes of agate that had been so often used, and

I had the pleasure of finding the results precisely the same.

To detail any more operations of this kind will be unnecessary; all the facts prove that the fixed alkali is not *generated*, but *evolved*, either from the solid materials employed, or from saline matter in the water.

I have made many experiments in vessels composed of different substances, with the water procured by very slow distillation: and in almost every instance some fixed alkali appeared.

In tubes of wax the alkaline matter was a mixture of soda and potash; and the acid matter a mixture of sulphuric, muriatic, and nitric acids.

In a tube of resin, the alkaline matter seemed to be principally potash.

A cube of Carrara marble of about an inch, having an aperture in its centre, was placed in a crucible of platina, which was filled as high as the upper surface of the cube with the purified water, the aperture was filled with the same fluid; the crucible was positively electrified by a strong VOLTAIC power, and a negatively electrified wire introduced into the aperture.

The water soon gained the property of affecting the tint of turmeric; and fixed alkali and lime were both obtained from it: and this effect took place in repeated experiments: the fixed alkali, however, diminished in quantity every time; and after eleven processes conducted from two to three hours each, disappeared altogether. The production of lime-water was uniform.

I made a solution of 500 grains of this marble in nitric

acid; I decomposed the mixture by carbonate of ammonia, and I collected and evaporated the fluid part, and decomposed the nitrate of ammonia by heat. About $\frac{3}{4}$ of a grain of fixed saline matter remained, which had soda for its base.

It was possible that the Carrara marble might have been recently exposed to sea-water; I therefore tried, in the same way, a piece of granular marble, which I had myself broken from a rock on one of the highest of the primitive mountains of Donegal. It afforded fixed alkali by the agency of negative electricity.

A piece of argillaceous schist from Cornwall, treated in the same manner, gave the same result; and serpentine from the Lizard, and grauwackè from North Wales, both afforded soda. It is probable that there are few stones that do not contain some minute portions of saline matter, which in many cases may be mechanically diffused through their substance: and it is not difficult to conceive the possibility of this, when we consider that all our common rocks and strata bear evident marks of having been anciently covered by the sea.

I was now able to determine distinctly, that the soda procured in glass tubes came principally from the glass, as I had always supposed.

I used the two cones of gold with the purified water and the amianthus; the process was conducted as usual. After a quarter of an hour, the negatively electrified tube did not change the colour of turmeric. I introduced into the top of it a bit of glass; in a few minutes the fluid at the surface rendered the tint of the paper of a deep bright brown.

I had never made any experiments, in which acid matter

having the properties of nitrous acid was not produced, and the longer the operation the greater was the quantity that appeared.

Volatile alkali likewise seemed to be always formed in very minute portions, during the first few minutes in the purified water in the gold cones, but the limit to its quantity was soon attained.

It was natural to account for both these appearances, from the combination of nascent oxygene and hydrogene respectively; with the nitrogene of the common air dissolved in the water: and Dr. PRIESTLEY's experiments on the absorption of gases by water (on this idea) would furnish an easy explanation of the causes of the constant production of the acid, and the limited production of the alkali: for hydrogene, during its solution in water, seems to expel nitrogene; whilst nitrogene and oxygene are capable of co-existing dissolved in that fluid.*

To render the investigation more complete, I introduced the two cones of gold with purified water under the receiver of an air pump; the receiver was exhausted till it contained only $\frac{1}{64}$ of the original quantity of air; and then, by means of a convenient apparatus, the tubes were connected with an active VOLTAIC pile of 50 pairs of plates of 4 inches square. The processs was carried on for 18 hours, when the result was examined. The water in the negative tube produced no effect upon prepared litmus, but that in the positive tube gave it a barely perceptible tinge of red.

An incomparably greater quantity of acid would have been formed in a similar time in the atmosphere, and the small

* PRIESTLEY's Experiments and Observations, Vol. I. page 59.

portion of nitrogene gas remaining in contact with the water, seemed adequate to the effect.

I repeated the experiment under more conclusive circumstances. I arranged the apparatus as before; I exhausted the receiver, and filled it with hydrogene gas from a convenient airholder; I made a second exhaustion, and again introduced hydrogene that had been carefully prepared. The process was conducted for 24 hours, and at the end of this time neither of the portions of the water altered in the slightest degree the tint of litmus.

It seems evident then that water, chemically pure, is decomposed by electricity into gaseous matter alone, into oxygene and hydrogene.

The cause of its decomposition, and of the other decompositions which have been mentioned, will be hereafter discussed.

III. *On the Agencies of Electricity in the Decomposition of various Compounds.*

The experiments that have been detailed on the production of alkali from glass, and on the decomposition of various saline compounds contained in animal and vegetable substances, offered some curious objects of enquiry.

It was evident, that in all changes in which acid and alkaline matter had been present, the acid matter collected in the water round the positively electrified metallic surface; and the alkaline matter round the negatively electrified metallic surface; and this principle of action appeared immediately related to one of the first phænomena observed in the VOLTAIC pile, the

decomposition of the muriate of soda attached to the paste-board; and to many facts which have been since observed on the separation of the constituent parts of neutrosaline and metallic solutions, particularly those detailed by M. M. HISINGER and BERZELIUS.*

The first experiments that I made immediately with respect to this subject were on the decomposition of solid bodies, insoluble, or difficultly soluble in water. From the effects of the electrical agency on glass, I expected that various earthy compounds would undergo change under similar circumstances; and the results of the trials were decided and satisfactory.

Two cups made of compact sulphate of lime, containing about 14 grain-measures of water each, were connected together by fibrous sulphate of lime, which was moistened by pure water: the cups were filled with this fluid; platina wires from the VOLTAIC battery of 100 pairs of plates of six inches were introduced into them, so that the circuit of electricity was through the fibrous sulphate of lime. In five minutes the water in the cup connected with the positive wire became acid; that in the opposite cup strongly tinged turmeric. After an hour the fluids were accurately examined; when it was found that a pure and saturated solution of lime had been produced in the cup containing the negative wire, which was partially covered with a crust of lime; and that the other cup was filled with a moderately strong solution of sulphuric acid.

I procured two cubical pieces of crystallized sulphate of strontites, of about an inch; a hole was drilled in each capable of containing about 8 grains of water: the cubes were

* *Annales de Chimie*, Tom. LI. p. 167.

plunged in pure water in a platina crucible; and the level of the fluid preserved a few lines below the surface of the cubes; two platina wires were introduced into the holes, which were filled with pure water. The disengagement of gas, when the wires were connected with the battery of 100, proved that the sulphate of strontites was sufficiently porous to form a proper conducting chain. The results were much longer in being obtained in this experiment than in the last: some time elapsed before a sensible effect could be perceived; but the termination was similar. In 30 hours the fluid in the cavity containing the negative wire had gained the property of precipitating solution of sulphate of potash; and the presence of sulphuric acid in the other cavity was evident from its effect upon solution of muriate of barytes.

I made an experiment upon fluate of lime under like circumsances; but the crystallized fluate not being equally permeable to moisture, the two cavities were connected by moist asbestus. This decomposition was likewise very slow; but in the course of two days a pretty strong solution of lime was obtained in one tube; and an acid fluid in the other, which precipitated acetite of lead, and left a spot upon the glass from which it had been evaporated.

Sulphate of barytes, as might be supposed, proved much more difficult of decomposition than either sulphate of strontites or fluate of lime. I had made four or five experiments upon it, with the same kind of apparatus that had been applied to the fluate of lime, before I was able to gain decided results. In the last process performed on this substance, two pieces of a large single crystal were hollowed by grinding, so as to contain about five grains of water each;

they were connected by moist asbestus, and constantly subjected during four days to the strong action of a battery of 150 pairs of plates of 4 inches square. As the water diminished, its place was supplied by new quantities. At the conclusion of the experiment the fluid on the positive side of the apparatus instantly reddened litmus, tasted very sour, and gave a distinct precipitate with a solution of muriate of barytes; the water on the other side deepened the tincture of turmeric; but did not render solution of sulphate of potash turbid. There was a small quantity of white crust, however, on the sides and the bottom of the cavity, and I conceived that this might be the barytes, which, during the extremely slow decomposition, would have combined with the carbonic acid of the atmosphere. To ascertain if this had been the case, I introduced into the cavity a drop of diluted muriatic acid; a slight effervescence appeared, and the fluid obtained occasioned a distinct white cloudiness in solution of sulphate of soda.

In all these cases the constituent parts of the bodies newly arranged by the effects of electricity existed in considerable quantities, and exposed on a large surface to its action. I had great reason to believe, however, from the trials with distilled water in different vessels, that very minute portions of acid and alkaline matter might be disengaged by this agency from solid combinations, principally consisting of the pure earths.

This part of the investigation was easily elucidated.

For a purpose of geological enquiry, which on a future occasion I shall have the honour of laying before the Society, I had made a careful analysis of a specimen of fine grained basalt from Port Rush in the county of Antrim, by means of

fusion with boracic acid: it had afforded in 100 parts $3\frac{1}{2}$ parts of soda, and nearly $\frac{1}{2}$ a part of muriatic acid, with 15 parts of lime. This stone appeared to me very well fitted for the purpose of experiment: cavities were drilled in two pieces, properly shaped; they contained about 12 grains of water each; they were connected by moistened amianthus, and the process conducted as usual with a power of 50 pairs of plates. At the end of ten hours the result was examined with care. The fluid that had been positively electrified had the strong smell of oxymuriatic acid, and copiously precipitated nitrate of silver; the other portion of fluid affected turmeric, and left by evaporation a substance which seemed to be a mixture of lime and soda.

A part of a specimen of compact zeolite, from the Giant's Causeway, which by analysis had given 7 parts in 100 of soda, had a small cavity made in it; it was immerged in pure water in a crucible of platina, and electrified in the same manner as the cube of Carrara marble, mentioned in page 9. In less than two minutes the water in the cavity had gained the property of changing the colour of turmeric; and in half an hour the solution was disagreeably alkaline to the taste. The matter dissolved proved to be soda and lime.

Lepidolite, treated in the same way, gave potash.

A piece of vitreous lava, from Etna, gave alkaline matter, which seemed to be a mixture of soda, potash, and lime.

As in these trials the object was merely to ascertain the general fact of decomposition, the process was never conducted for a sufficient time to develope a quantity of alkaline matter capable of being conveniently weighed, and of course any loss of weight of the substance could not be determined.

I thought it right, however, to make one experiment of this kind, for the sake of removing every possibility of doubt on the source of the different products; and I selected for this purpose glass, as a substance apparently insoluble in water, and not likely to afford in any way erroneous results.

The balance that I employed was made for the Royal Institution, by Mr. FIDLER, after the model of that belonging to the Royal Society; it turns readily with $\frac{1}{500}$ of a grain when loaded with 100 grains on each side; a glass tube with a platina wire attached, weighing 84 grains $\frac{56}{128}$ was connected with an agate cup, by amianthus; they were filled with purified water, and electrified by a power from 150 pairs of plates, in such a way that the platina in the glass tube was negative. The process was continued for 4 days, when the water was found alkaline. It gave by evaporation and exposure to a heat of about 400° FAHRENHEIT, soda mixed with a white powder insoluble in acids, the whole weight of which was $\frac{36}{128}$ of a grain. The glass tube carefully cleaned and dried weighed 84 grains, $\frac{37}{128}$. The difference between the loss of weight of the tube and the weight of the products in the water may be easily explained: some minute detached particles of amianthus were present, and the soda must have contained water, a substance which it is probably perfectly free from in glass.

Having obtained such results with regard to the disengagement of the saline parts of bodies insoluble in water, I made a number of experiments on soluble compounds; their decomposition was always much more rapid, and the phænomena perfectly distinct.

In these processes I employed the agate cups with platina wires, connected by amianthus moistened in pure water; the

solutions were introduced into the cups, and the electrifying power applied from batteries of 50 pairs of plates, in the usual way.

A diluted solution of sulphate of potash treated in this manner, produced in four hours at the negative wire a weak lixivium of potash; and a solution of sulphuric acid at the positive wire.

The phænomena were similar when sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate oxalate, and benzoate of ammonia, and alum were used. The acids in a certain time collected in the tube containing the positive wire, and the alkalies and earths in that containing the negative wire.

Solutions of the muriatic salts, decomposed in the same way, uniformly gave oxymuriatic acid on the positive side.

When compatible mixtures of neutrosaline solutions containing the common mineral acids were used, the different acids and the different bases seemed to separate together in a mixed state, without any respect to the orders of affinity.

When metallic solutions were employed, metallic crystals or depositions were formed, as in common GALVANIC experiments, on the negative wire, and oxide was likewise deposited round it; and a great excess of acid was soon found in the opposite cup. With solutions of iron, zinc, and tin, this effect took place, as well as with the more oxidable metals: when muriate of iron was used, the black substance deposited upon the wire was magnetic, and dissolved with effervescence in muriatic acid; and when sulphate of zinc was used, a gray powder possessed of the metallic lustre, and likewise soluble with effervescence, appeared; and in all cases acid in excess was exhibited on the positive side.

Strong or saturated saline solutions, as might have been expected, afforded indications of the progress of decomposition much more rapidly than weak ones; but the smallest proportion of neutrosaline matter seemed to be acted on with energy.

A very simple experiment demonstrates this last principle. If a piece of paper tinged with turmeric, is plunged into pure water in a proper circuit, in contact with the negative point, the very minute quantity of saline compound contained in the paper, affords alkaline matter sufficient to give it instantly a brown tint near its point of contact: and acid in the same manner is immediately developed from litmus paper, at the positive surface.

I made several experiments, with the view of ascertaining whether, in the decompositions by electricity, the separation of the constituent parts was complete from the last portions of the compound; and whenever the results were distinct, this evidently appeared to be the case.

I shall describe one of the most conclusive of the experiments: a very weak solution of sulphate of potash, containing 20 parts water and one part saturated solution, at 64°, was electrified in the two agate cups by the power of 50 pairs of plates for three days: the connecting amianthus, which had been moistened with pure water, was removed, washed with pure water, and again applied, twice every day; by this precaution the presence of any neutral salt that might adhere to it, and disturb the results, was prevented. The alkali obtained in this process in the solution had the properties of pure potash; and when it had been saturated with nitric acid it gave no turbidness by mixture with solution of muriate of

barytes: the acid matter exposed to a strong heat evaporated without leaving any residuum.

IV. *On the Transfer of certain of the constituent Parts of Bodies by the Action of Electricity.*

M. GAUTHEROT has stated,* that in a single GALVANIC circle of zinc, silver, and water, in an active state, the oxide of zinc formed is attracted by the silver;† and M. M. HISINGER and BERZELIUS detail an account of an experiment, in which solution of muriate of lime being placed in the positive part of a siphon, electrified by wires from a VOLTAIC pile, and distilled water in the negative part, lime appeared in the distilled water.

These facts rendered it probable, that the saline elements evolved in decompositions by electricity were capable of being transferred from one electrified surface to another, according to their usual order of arrangement; but to demonstrate this clearly, new researches were wanting.

I connected one of the cups of sulphate of lime, mentioned page 13, with a cup of agate by asbestus; and, filling them with purified water, made the platina wire in the cup of sulphate of lime transmit the electricity from a power of 100; a wire in the agate cup received it. In about four hours a strong solution of lime was found in the agate cup, and sulphuric acid in the cup of sulphate of lime. By reversing the order, and carrying on the process for a similar time, the sulphuric acid appeared in the agate cup, and the solution of lime on the opposite side.

Many trials were made with other saline substances, with

* *Annales de Chimie*, Vol. XXXIX. page 203. † Ibid. Vol. LI. page 173.

analogous results. When the compounds of the strong mineral acids with alkaline or alkaline-earthy bases, were introduced into one tube of glass, distilled water connected by amianthus being in another tube, both connected by wires of platina in the VOLTAIC arrangement, the base always passed into the distilled water when it was negative, and the acid when it was positive.

The metals and the metallic oxides passed towards the negative surface like the alkalies, and collected round it. In a case in which solution of nitrate of silver was used on the positive side, and distilled water on the negative, silver appeared on the whole of the transmitting amianthus, so as to cover it with a thin metallic film.

The time required for these transmissions (the quantity and intensity of the electricity, and other circumstances remaining the same) seemed to be in some proportion as the length of the intermediate volume of water. Thus when with the power of 100, sulphate of potash was on the negative side, and distilled water on the positive side, the distance between the wires being only an inch, sulphuric acid, in sufficient quantity to be very manifest, was found in the water in less than five minutes: but when the tubes were connected by an intermediate vessel of pure water, so as to make the circuit 8 inches, 14 hours were required to produce the same effect.

To ascertain whether the contact of the saline solution with a metallic surface was necessary for the decomposition and transfer, I introduced purified water into two glass tubes; a vessel containing solution of muriate of potash was connected with them respectively by amianthus; and the arrangement was made in such a way, that the level of both the portions

of purified water was higher than the level of the saline solution.

In this case, the saline matter was distant from each of the wires at least $\frac{2}{3}$ of an inch; yet alkaline matter soon appeared in one tube, and acid matter in the other: and in 16 hours moderately strong solutions of potash, and of muriatic acid had been formed.

In this case of electrical transfer or attraction, the acid and alkaline matter seemed to be perfectly pure; and I am inclined to believe that this is uniformly the case in all experiments carefully made. One of the instances in which I conceived acid most likely to be present, was in the transfer of magnesia from sulphate of magnesia in the positive tube, to distilled water in the negative tube. I examined the case, taking care that the distilled water was never upon a lower level than the saline solution: the process was continued for some hours, till a considerable quantity of magnesia had appeared. The connecting amianthus was removed, and muriatic acid poured into the tube: the saturated solution did not precipitate solution of muriate of barytes.

I endeavoured to ascertain the progress of the transfer, and the course of the acid or alkaline matter in these decompositions, by using solutions of litmus and turmeric, and papers coloured by these substances; and these trials led to the knowledge of some singular and unexpected circumstances.

Two tubes, one containing distilled water, the other solution of sulphate of potash, were each connected by amianthus with a small *oz.* measure filled with distilled water tinged by litmus: the saline solution was negatively electrified; and as it was natural to suppose, that the sulphuric acid in passing

through the water to the positive side would redden the litmus in its course, some slips of moistened paper tinged with litmus were placed above and below the pieces of amianthus, directly in the circuit. The progress of the experiment was minutely observed; the first effect of reddening took place immediately above the positive surface, where I had least expected it; the red tint slowly diffused itself from the positive side to the middle of the vessel, but no redness appeared above the amianthus, or about it, on the negative side, and though it had been constantly transmitting sulphuric acid, it remained unaffected to the last.

The order of the experiment was changed, and the saline solution placed on the positive side; a solution and papers tinged with turmeric being substituted for those tinged with litmus. The effect was precisely analogous; the turmeric became brown first, near the negative wire, and no change took place in the intermediate vessel near the positive wire.

In another process, the two glass tubes were filled with solution of muriate of soda, and the intermediate vessel with solution of sulphate of silver; paper tinged with turmeric was placed on the positive side, and paper tinged with litmus on the negative side; as soon as the electrical circuit was complete, soda began to appear in the negative tube, and oxy-muriatic acid in the positive tube; and the alternate products were exhibited passing into the solution of sulphate of silver, the muriatic acid occasioning a dense heavy precipitate, and the soda a more diffused and a lighter one; but neither the turmeric transmitting the alkali, nor the litmus transmitting the acid, had their tints in the slightest degree altered.

V. *On the Passage of Acids, Alkalies, and other Substances through various attracting chemical Menstrua, by Means of Electricity.*

As acid and alkaline substances during the time of their electrical transfer passed through water containing vegetable colours without affecting them, or apparently combining with them, it immediately became an object of enquiry, whether they would not likewise pass through chemical menstrua, having stronger attractions for them; and it seemed reasonable to suppose, that the same power which destroyed elective affinity in the vicinity of the metallic points, would likewise destroy it, or suspend its operation, throughout the whole of the circuit.

An arrangement was made, of the same vessels and apparatus employed in the experiment on the solution of muriate of soda and sulphate of silver, page 23. Solution of sulphate of potash was placed in contact with the negatively electrified point, pure water was placed in contact with the positively electrified point, and a weak solution of ammonia was made the middle link of the conducting chain; so that no sulphuric acid could pass to the positive point in the distilled water, without passing through the solution of ammonia.

The power of 150 was used: in less than five minutes it was found, by means of litmus paper, that acid was collecting round the positive point; in half an hour, the result was sufficiently distinct for accurate examination.

The water was sour to the taste, and precipitated solution of nitrate of barytes.

Similar experiments were made with solution of lime; and

weak solutions of potash and soda, and the results were analogous. With strong solutions of potash and soda a much longer time was required for the exhibition of the acid; but even with the most saturated alkaline lixivium, it always appeared in a certain period.

Muriatic acid, from muriate of soda, and nitric acid from nitrate of potash, were transmitted through concentrated alkaline menstrua, under similar circumstances.

When distilled water was placed in the negative part of the circuit, and a solution of sulphuric, muriatic, or nitric acid, in the middle, and any neutral salt with a base of lime, soda, potash, ammonia, or magnesia, in the positive part, the alkaline matter was transmitted through the acid matter to the negative surface, with similar circumstances to those occurring during the passage of the acid through the alkaline menstrua; and the less concentrated the solution, the greater seemed to be the facility of transmission.

I tried in this way muriate of lime with sulphuric acid, nitrate of potash with muriatic acid, sulphate of soda with muriatic acid, and muriate of magnesia with sulphuric acid; I employed the power of 150; and in less than 48 hours, I gained in all these cases decided results; and magnesia came over like the rest.

Strontites and barytes passed, like the other alkaline substances, readily through muriatic and nitric acids; and, *vice versâ*, these acids passed with facility through aqueous solutions of barytes and strontites; but in experiments in which it was attempted to pass sulphuric acid through the *same menstrua*, or to pass barytes or strontites through this acid, the results were very different.

When solution of sulphate of potash was in the negative part of the circuit, distilled water in the positive part, and saturated solution of barytes in the middle, no sensible quantity of sulphuric acid existed in the distilled water after 30 hours, the power of 150 being used; after four days, sulphuric acid appeared, but the quantity was extremely minute; much sulphate of barytes had formed in the intermediate vessel; the solution of barytes was so weak as barely to tinge litmus; and a thick film of carbonate of barytes had formed on the surface of the fluid. With solution of strontites the result was very analogous, but the sulphuric acid was sensible in three days.

When solution of muriate of barytes was made positive by the power of 150, concentrated sulphuric acid intermediate, and distilled water negative. No barytes appeared in the distilled water, when the experiment had been carried on for four days; but much oxymuriatic acid had formed in the positive vessel, and much sulphate of barytes had been deposited in the sulphuric acid.

Such of the metallic oxides as were made subjects of experiment passed through acid solutions from the positive to the negative side, but the effect was much longer in taking place than in the instances of the transition of alkaline matter. When solution of green sulphate of iron was made positive, solution of muriatic acid intermediate, and water negative, in the usual arrangement, green oxide of iron began to appear in about ten hours upon the negative connecting amianthus, and in three days a considerable portion had been deposited in the tube. Analogous results were obtained with sulphate of copper, nitrate of lead, and nitromuriate of tin.

I made several experiments on the transition of alkaline and acid matter through different neutrosaline solutions, and the results were such as might well have been anticipated.

When solution of muriate of barytes was negative, solution of sulphate of potash intermediate, and pure water positive, the power being from 150, sulphuric acid appeared in about five minutes in the distilled water; and in two hours the muriatic acid was likewise very evident. When solution of sulphate of potash was positive, solution of muriate of barytes intermediate, and distilled water negative, the barytes appeared in the water in a few minutes; the potash from the more remote part of the chain was nearly an hour in accumulating, so as to be sensible.

When the solution of muriate of barytes was positive, the solution of sulphate of potash intermediate, and distilled water negative, the potash soon appeared in the distilled water; a copious precipitation of sulphate of barytes formed in the middle vessel; but after ten hours no barytes had passed into the water.

When solution of sulphate of silver was interposed between solution of muriate of barytes on the negative side, and pure water on the positive side, sulphuric acid alone passed into the distilled water; and there was a copious precipitation in the solution of sulphate of silver. This process was carried on for ten hours.

I tried several of these experiments of transition upon vegetable and animal substances with perfect success.

The saline matter exposed in contact with the metal, and that existing in the vegetable or animal substances, both underwent decomposition and transfer; and the time of the appearance of the different products at the extremities of the circuit was governed by the degree of their vicinity.

Thus, when a fresh leaf-stalk of the polyanthus, about 2 inches long, was made to connect a positively electrified tube containing solution of nitrate of strontites, and a negatively electrified tube containing pure water. The water soon became green, and gave indications of alkaline properties, and free nitric acid was rapidly separated in the positive tube. After ten minutes, the alkaline matter was examined; it consisted of potash and lime, and as yet no strontites had been carried into it: for the precipitate it gave with sulphuric acid readily dissolved in muriatic acid. In half an hour strontites, however, appeared; and in four hours it formed a very abundant ingredient of the solution.

A piece of muscular flesh of beef, of about 3 inches in length and half an inch in thickness, was treated in the same way as the medium of communication between muriate of barytes and distilled water. The first products were soda, ammonia, and lime; and after an hour and a quarter, the barytes was very evident. There was much free oxymuriatic acid in the positively electrified tube, but no particle of muriatic acid had passed into the negative tube, either from the muriatic solution or from the muscular fibre.

VI. *Some general Observations on these Phænomena, and on the Mode of Decomposition and Transition.*

It will be a general expression of the facts that have been detailed, relating to the changes and transitions by electricity, in common philosophical language, to say, that hydrogene, the alkaline substances, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces, and repelled by positively electrified metallic surfaces; and con-

trariwise, that oxygene and acid substances are attracted by positively electrified metallic surfaces, and repelled by negatively electrified metallic surfaces; and these attractive and repulsive forces are sufficiently energetic to destroy or suspend the usual operation of elective affinity.

It is very natural to suppose, that the repellent and attractive energies are communicated from one *particle to another particle* of the same kind, so as to establish a conducting chain in the fluid; and that the locomotion takes place in consequence; and that this is really the case seems to be shown by many facts. Thus, in all the instances in which I examined alkaline solutions through which acids had been transmitted, I always found acid in them whenever any acid matter remained at the original source. In time, by the attractive power of the positive surface, the decomposition and transfer undoubtedly become complete; but this does not affect the conclusion.

In the cases of the separation of the constituents of water, and of solutions of neutral salts forming the whole of the chain, there may possibly be a succession of decompositions and recompositions throughout the fluid. And this idea is strengthened by the experiments on the attempt to pass barytes through sulphuric acid, and muriatic acid through solution of sulphate of silver, in which, as insoluble compounds are formed and carried out of the sphere of the electrical action, the power of transfer is destroyed. A similar conclusion might likewise be drawn from many other instances. Magnesia and the metallic oxides, as I have already mentioned, will pass along moist amianthus from the positive to the negative surface; but if a vessel of pure water be interposed, they do not reach the negative vessel, but sink to the bottom.

These experiments I have very often made, and the results are perfectly conclusive; and in the case, page 26, in which sulphuric acid seemed to pass in small quantities through very weak solutions of strontites and barytes, I have no doubt but that it was carried through by means of a thin stratum of pure water, where the solution had been decomposed at the surface by carbonic acid; for in an experiment similar to these in which the film of carbonate of barytes was often removed and the fluid agitated, no particle of sulphuric acid appeared in the positive part of the chain.

It is easy to explain, from the general phænomena of decomposition and transfer, the mode in which *oxygene* and *hydrogene* are separately evolved from water. The oxygene of a portion of water is attracted by the positive surface, at the same time that the other constituent part, the hydrogene is repelled by it; and the opposite process takes place at the negative surface; and in the middle or neutral point of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only are active, there must be a new combination of the repelled matter: and the case is analogous to that of two portions of muriate of soda separated by distilled water; muriatic acid is repelled from the negative side, and soda from the positive side, and muriate of soda is composed in the middle vessel.

These facts seem fully to invalidate the conjectures of M. RITTER, and some other philosophers, with regard to the elementary nature of water, and perfectly to confirm the great discovery of Mr. CAVENDISH.

M. RITTER conceived that he had procured oxygene from water without hydrogene, by making sulphuric acid the medium of communication at the negative surface; but in this case,

sulphur is deposited, and the oxygene from the acid, and the hydrogene from the water are respectively repelled; and a new combination produced.

I have attempted some of the experiments of decomposition and transfer, by means of common electricity, making use of a powerful electrical machine of Mr. NAIRNE's construction, belonging to the Royal Institution, of which the cylinder is 15 inches in diameter, and 2 feet long.

With the same apparatus as that employed for decompositions by the VOLTAIC battery, no perceptible effect was produced by passing a strong current of electricity silently for four hours through solution of sulphate of potash.

But by employing fine platina points of $\frac{1}{70}$ of an inch in diameter, cemented in glass tubes in the manner contrived by Dr. WOLLASTON,* and bringing them near each other, in vessels containing from 3 to 4 grains of the solution, and connected by moist asbestus, potash appeared in less than two hours round the negatively electrified point, and sulphuric acid round the positive point.

In a similar experiment sulphuric acid was transferred through moist asbestus into water; so that there can be no doubt, that the principle of action is the same in common and the VOLTAIC electricity.†

* Phil. Trans. Vol. XCI. page 427.

† This had been shewn, with regard to the decomposition of water, by Dr. WOLLASTON's important researches. — By carefully avoiding sparks, I have been able to obtain the two constituents in a separate state. In an experiment in which a fine platina point cemented in glass, and connected by a single wire with the positive conductor of this machine, was plunged in distilled water in an insulated state, and the electricity dissipated into the atmosphere by means of moistened filaments of cotton, oxygene gas, mixed with a little nitrogene gas, was produced; and when the same apparatus was applied to the negative conductor hydrogene gas was evolved, and a minute

VII. *On the general Principles of the chemical Changes produced by Electricity*

The experiments of Mr. BENNET had shown, that many bodies brought into contact and afterwards separated, exhibited *opposite* states of electricity; but it is to the investigations of VOLTA that a clear developement of the fact is owing; he has distinctly shown it in the case of copper and zinc, and other metallic combinations; and has supposed that it also takes place with regard to metals and fluids.

In a series of experiments made in 1801,* on the construction of electrical combinations by means of alternations of single metallic plates, and different strata of fluids, I observed that when acid and alkaline solutions were employed as elements of these instruments, the alkaline solutions always received the electricity from the metal, and the acid always transmitted it to the metal; thus, in an arrangement of which the elements were tin, water, and solution of potash, the circulation of the electricity was from the water to the tin, and from the tin to the solution of potash; but in an arrangement composed of weak nitric acid, water, and tin; the order was from the acid to the tin, and from the tin to the water.

These principles seem to bear an immediate relation to the general phænomena of decomposition and transference, which have been the subject of the preceding details.

portion of oxygene and nitrogene gases: but neither of the foreign products, the nitrogene gas in the one case and the nitrogene and oxygene gases in the other, formed as much as $\frac{1}{30}$ part of the volume of the gases; and there is every reason to suppose that they were derived from the extrication of common air, which had been dissolved in the water. This result, which when I first obtained it in 1803, appeared very obscure, is now easily explained; the alternate products must have been evolved at the points of the dissipation of the electricity.

* See Phil. Trans. Vol. XCI. page 397.

In the simplest case of electrical action, the alkali which receives electricity from the metal would necessarily, on being separated from it, appear positive; whilst the acid under similar circumstances would be negative; and these bodies having respectively with regard to the metals, that which may be called a positive and a negative electrical energy, in their repellent and attractive functions seem to be governed by laws the same as the common laws of electrical attraction and repulsion. The body possessing the positive energy being repelled by positively electrified surfaces, and attracted by negatively electrical surfaces; and the body possessing the negative energy following the contrary order.

I have made a number of experiments with the view of elucidating this idea, and of extending its application; and in all cases they have tended to confirm the analogy in a remarkable manner.

Well burned charcoal water and nitric acid, the same substance water and solution of soda, made respectively elements of different electrical combinations, became distinctly active when 20 alternations were put together: the positive energy being exhibited on the side of the alkali, and the negative on that of the acid. Arrangements of plates of zinc, pieces of moistened pasteboard, and moistened quicklime, to the number of 40 series, likewise formed a weak electrical pile, the effect of the lime being similar to that of an alkali, but the power was soon lost.

I endeavoured, by means of very delicate instruments, to ascertain the electrical states of single insulated acid and alkaline solutions, after their contact with metals; and for this purpose I employed at different times the condensing

electrometer of Mr. CUTHBERTSON'S construction, Mr. CAVALLO'S multiplier, and a very sensible electrical balance, on the principle of torsion, adopted by M. COULOMB; but the effects were unsatisfactory, the circumstances of evaporation, and of chemical action, and the adherence of the solutions to the surfaces of the metals employed, in most cases, prevented any distinct result, or rendered the source of the electricity doubtful. I shall not enter into any details of these processes, or attempt to draw conclusions from capricious and uncertain appearances, which, as we shall immediately see, may be fully deduced from clear and distinct ones.

The alkaline and acid substances capable of existing in the dry and solid form, give by contact with the metals exceedingly sensible electricities, which require for their exhibition the gold leaf electrometer only with the small condensing plate.

When oxalic, succinic, benzoic or boracic acid, perfectly dry, either in powder or crystals, were touched upon an extended surface with a plate of copper insulated by a glass handle, the copper was found positive, the acid negative. In favourable weather, and when the electrometer was in perfect condition, one contact of the metal was sufficient to produce a sensible charge; but seldom more than five or six were required. Other metals, zinc, and tin for instance were tried with the same effect. And the metal received the positive charge, apparently to the same extent, whether the acid was insulated upon glass, or connected with the ground.

The solid acid of phosphorus, which had been strongly ignited, and most carefully excluded from the contact of air, rendered the insulated plate of zinc positive by four contacts;

but after exposure to the atmosphere for a few minutes it wholly lost this power..

When metallic plates were made to touch dry lime, strontites, or magnesia, the metal became negative; the effect was exceedingly distinct, a single contact upon a large surface being sufficient to communicate a considerable charge. For these experiments the earths were carefully prepared; they were in powder, and had been kept for several days in glass bottles before they were used: it is essential to the success of the process that they be of the temperature of the atmosphere. In some experiments which I made upon them when cooling, after having been ignited; they appeared strongly electrical, and rendered the conductors brought in contact with them positive.

I made several experiments in a similar manner on the effects of the contact of potash and soda with the metals. Potash in no instance afforded a satisfactory result; its powerful attraction for water presents an obstacle probably unsurmountable to the success of any trials made in the free atmosphere. Soda, in the only case in which electricity was exhibited, affected the metal in the same way as lime, strontites, and magnesia. Upon this occasion the soda had been prepared with great care, exposed in a platina crucible for nearly an hour in a red heat, and suffered to cool in the crucible inverted over mercury; when cool it was immediately removed, and the contact made with a plate of zinc: the experiment was performed in the open air; the weather was peculiarly dry, the thermometer stood at 28° FAHRENHEIT, and the barometer at 30.2 inches; six contacts gave a charge to the condensing electrometer in the first trial; in the second ten were required to produce a similar effect; and after this,

though two minutes only had elapsed, no farther result could be obtained.

In the decomposition of sulphuric acid by VOLTAIC electricity the sulphur separates on the negative side. The experiments of various electricians prove, that by the friction of sulphur and metals, the sulphur becomes positive and the metals negative; the same thing I find happens from the contact of an unexited cake of sulphur and insulated metallic plates. Mr. WILKE has stated an exception to lead, as rendering sulphur negative by its friction. The results that I have obtained with lead, in trials very carefully made, are the same as those with other metals.* Sulphur, by being rubbed or struck against newly-polished lead, always became positive. Mr. WILKE perhaps was misled by using tarnished lead: sulphur, I find, rubbed against litharge, or lead the surface of which has been long exposed to air, becomes negative; and this exception being removed, all the facts on the subject are confirmations of the general principle.†

* As sulphur is a nonconductor, and easily excited by slight friction, or small changes in its temperature, some caution is required in drawing conclusions from the experiments in which it is employed. Sulphur, examined immediately after having been heated, gives a positive charge to conductors, agreeing in this respect with the alkaline substances; and a slight contact with the dry hand is sufficient to render it negative. In general likewise in experiments of contact care should be taken that the metallic plate is free from electricity: well polished plates of copper and zinc will, I find, receive a negative charge from being laid on a table of common mahogany.

† Concentrated solution of phosphoric acid, I find, is decomposed by VOLTAIC electricity: the phosphorus combines with the negatively electrified metal, and forms a phosphuret; at least this happened in the two cases that I tried with platina and copper. From all analogy it may be inferred, that the electrical energy of this inflammable substance with regard to metals is the same as that of sulphur. I tried some experiments of contact upon it, but without success. Its slow combustion in the atmosphere it is most likely was the cause of the failure: but even in gases not containing free or loosely combined oxygene, its evaporation would probably interfere.

On the general principle, oxygene and hydrogene ought to possess, with regard to the metals respectively, the negative and positive energy. This I have not been able to prove by direct experiments of contact; but the idea is confirmed by the agency of their compounds; thus I have found that solution of sulphuretted hydrogene in water acts in the electrical apparatus composed of single plates and different strata of fluids, in the same manner as alkaline solutions; and that solution of oxymuriatic acid is more powerful in similar arrangements than solutions of muriatic acid of a higher degree of concentration; and in both these cases, it is impossible to conceive the combined hydrogene and oxygene inactive. The inference likewise is fully warranted by the case of the solutions of alkaline hydroguretted sulphurets, which consisting principally of alkali and sulphur together in union with water, exhibit the positive energy with regard to the metals in a very high degree. In the series of experiments on VOLTAIC arrangements constructed with single plates abovementioned, I found the solutions of hydroguretted sulphurets in general much more active than alkaline solutions, and particularly active with copper, silver, and lead. And in an experiment that I made on a combination of copper, iron, and hydroguretted sulphurets of potash, in 1802, I found that the positive energy of the hydroguretted sulphurets with regard to the copper was sufficient to overpower that of the iron; so that the electricity did not circulate from the copper to the iron, and from the iron to the fluid, as in common cases, but from the copper to the hydroguretted sulphuret, and from the hydroguretted sulphuret to the iron.

All these details afford the strongest confirmation of the

principle. It may be considered almost as a mere arrangement of facts; and with some extensions it seems capable of being generally applied.

Bodies possessing opposite electrical energies with regard to one and the same body, we might fairly conclude would likewise possess them with regard to each other. This I have found by experiment is the case with lime and oxalic acid. A dry piece of lime, made from a very pure compact secondary limestone, and of such a form as to present a large smooth surface, became positively electrical by repeated contacts with crystals of oxalic acid: and these crystals placed upon the top of a condensing electrometer, and repeatedly touched by the lime, which after each contact was freed from its charge, rendered the gold leaves negatively electrical. The tendency of the mere contacts of the acid and alkali with the metal would be to produce opposite effects to those exhibited, so that their mutual agency must have been very energetic.

It will not certainly be a remote analogy to consider the other acid and alkaline substances generally, and oxygene and hydrogene as possessing similar electrical relations; and in the decompositions and changes presented by the effects of electricity, the different bodies naturally possessed of chemical affinities appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order. Thus, as we have seen, the acids in the positive part of the circuit, separate themselves from alkalies, oxygene from hydrogene, and so on; and metals on the negative side, do not unite to oxygene, and acids do not remain in union with their oxides; and in this way the attrac-

tive and repellent agencies seem to be communicated from the metallic surfaces throughout the whole of the menstruum.

VIII. *On the Relations between the electrical Energies of Bodies, and their chemical Affinities.*

As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state different from that which it naturally possesses; that is, by bringing it artificially into a state similar to the other, so it may be increased by exalting its natural energy. Thus, whilst zinc, one of the most oxidable of the metals, is incapable of combining with oxygene when negatively electrified in the circuit, even by a feeble power; silver, one of the least oxidable, easily unites to it when positively electrified; and the same thing might be said of other metals.

Amongst the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite states; thus, copper and zinc, gold and quicksilver, sulphur and the metals, the acid and alkaline substances, afford apposite instances; and supposing perfect freedom of motion in their particles or elementary matter, they ought, according to the principles laid down, to attract each other in consequence of their electrical powers. In the present state of our knowledge, it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with it, and an essential property of matter?

The coated glass plates of BECCARIA strongly adhere to each other when oppositely charged, and retain their charges on being separated. This fact affords a distinct analogy to the subject; different particles in combining must still be supposed to preserve their peculiar states of energy.

In the present early stage of the investigation, it would be improper to place unbounded confidence in this hypothesis; but it seems naturally to arise from the facts, and to coincide with the laws of affinity, so ably developed by modern chemists; and the general application of it may be easily made.

Supposing two bodies, the particles of which are in different electrical states, and those states sufficiently exalted to give them an attractive force superior to the power of aggregation, a combination would take place which would be more or less intense according as the energies were more or less perfectly balanced; and the change of properties would be correspondently proportional.

This would be the simplest case of chemical union. But different substances have different degrees of the same electrical energy in relation to the same body: thus the different acids and alkalies are possessed of different energies with regard to the same metal; sulphuric acid, for instance, is more powerful with lead than muriatic acid, and solution of potash is more active with tin than solution of soda. Such bodies likewise may be in the same state or repellent with regard to each other, as apparently happens in the cases just mentioned; or they may be neutral; or they may be in opposite or attracting states, which last seems to be the condition of sulphur and alkalies that have the same kind of energy with regard to metals.

When two bodies repellent of each other act upon the same body with different degrees of the same electrical attracting energy, the combination would be determined by the degree; and the substance possessing the weakest energy would be repelled; and this principle would afford an expression of the causes of elective affinity, and the decompositions produced in consequence.

Or where the bodies having different degrees of the same energy, with regard to the third body, had likewise different energies with regard to each other, there might be such a balance of attractive and repellent powers as to produce a triple compound; and by the extension of this reasoning, complicated chemical union may be easily explained.

Numerical illustrations of these notions might be made without difficulty, and they might be applied to all cases of chemical action; but in the present state of the enquiry, a great extension of this hypothetical part of the subject would be premature.

The general idea will, however, afford an easy explanation of the influence of affinity by the masses of the acting substances, as elucidated by the experiments of M. Bertholle t; for the combined effect of many particles possessing a feeble electrical energy, may be conceived equal or even superior to the effect of a few particles possessing a strong electrical energy: and the facts mentioned, page 25, confirm the supposition: for concentrated alkaline lixivia resist the transmission of acids by electricity much more powerfully than weak ones.

Allowing combination to depend upon the balance of the natural electrical energies of bodies, it is easy to conceive that

a *measure* may be found of the artificial energies, as to intensity and quantity produced in the common electrical machine, or the VOLTAIC apparatus, capable of destroying this equilibrium; and such a measure would enable us to make a scale of electrical powers corresponding to degrees of affinity.

In the circuit of the VOLTAIC apparatus, completed by metallic wires and water, the strength of the opposite electricities diminish from the points of contact of the wires towards the middle point in the water, which is necessarily neutral. In a body of water of considerable length it probably would not be difficult to assign the places in which the different neutral compounds yielded to, or resisted, decomposition. Sulphate of barytes, in all cases that I tried, required immediate contact with the wire: solution of sulphate of potash exhibited no marks of decomposition with the power of 150, when conconnected in a circuit of water ten inches in length, at four inches from the positive point; but when placed within two inches, its alkali was slowly repelled and its acid attracted.*

Whenever bodies brought by artificial means into a high state of opposite electricities are made to restore the equilibrium, heat and light are the common consequences. It is

* In this experiment, the water was contained in a circular glass bason two inches deep, the communication was made by pieces of amianthus of about the eighth of an inch in breadth. The saline solution filled a half ounce measure, and the distance between the solution and the water, at both points of communication, was a quarter of an inch. I mention these circumstances because the quantity of fluid and the extent of surface materially influence the result in trials of this kind. Water included in glass siphons forms a much less perfect conducting chain than when diffused upon the surface of fibrous nonconducting substances of much smaller volume than the diameter of the siphons. I attempted to employ siphons in some of my first experiments; but the very great inferiority of effect as compared with that of amianthus made me altogether relinquish the use of them.

perhaps an additional circumstance, in favour of the theory to state, that heat and light are likewise the result of all intense chemical action. And as in certain forms of the VOLTAIC battery, where large quantities of electricity of low intensity act, heat is produced without light; so in slow combinations there is an increase of temperature without luminous appearance.

The *effect of* HEAT, in producing combination, may be easily explained according to these ideas. It not only often gives more freedom of motion to the particles, but in a number of cases it seems to exalt the electrical energies of bodies; glass, the tourmalin, sulphur, all afford familiar instances of this last species of energy.

I heated together an insulated plate of copper and a plate of sulphur, and examined their electricities as their temperature became elevated: these electricities, scarcely sensible at 56° FAHRENHEIT to the condensing electrometer, became at 100° FAHRENHEIT capable of affecting the gold leaves without condensation; they increased in a still higher ratio as the sulphur approached towards its point of fusion. At a little above this point, as is well known from the experiments of the Dutch chemists, the two substances rapidly combine, and heat and light are evident.

Similar effects may be conceived to occur in the case of oxygene and hydrogene, which form water, a body apparently neutral in electrical energy to most other substances: and we may reasonably conclude that there is the same exaltation of power, in all cases of combustion. In general, when the different energies are strong and in perfect equilibrium, the combination ought to be quick, the heat and light intense, and

the new compound in a neutral state. This would seem to be the case in the instance just quoted; and in the circumstances of the union of the strong alkalies and acids. But where one energy is feeble and the other strong, all the effects must be less vivid; and the compound, instead of being neutral, ought to exhibit the excess of the stronger energy.

This last idea is confirmed by all the experiments which I have been able to make on the energies of the saline compounds with regard to the metals. Nitrate and sulphate of potash, muriate of lime, oxymuriate of potash, though repeatedly touched upon a large surface by plates of copper and zinc, gave no electrical charge to them; subcarbonate of soda and borax, on the contrary, gave a slight negative charge, and alum and superphosphate of lime a feeble positive charge.

Should this principle on further enquiry be found to apply generally, the degree of the electrical energies of bodies, ascertained by means of sensible instruments, will afford new and useful indications of their composition.

IX. *On the Mode of Action on the Pile of* VOLTA, *with experimental Elucidations.*

The great tendency of the attraction of the different chemical agents, by the positive and negative surfaces in the VOLTAIC apparatus, seems to be to restore the electrical equilibrium. In a VOLTAIC battery, composed of copper, zinc, and solution of muriate of soda, all circulation of the electricity ceases, the equilibrium is restored if copper be brought in contact with the zinc on both sides: and oxygene and acids,

which are attracted by the positively electrified zinc, exert similar agencies to the copper, but probably in a slighter degree, and being capable of combination with the metal, they produce a momentary equilibrium only.

The electrical energies of the metals with regard to each other, or the substances dissolved in the water, in the VOLTAIC and other analogous instruments, seem to be the causes that disturb the equilibrium, and the chemical changes the causes that tend to restore the equilibrium; and the phænomena most probably depend on their joint agency.

In the VOLTAIC pile of zinc, copper, and solution of muriate of soda, in what has been called its condition of electrical tension, the communicating plates of copper and zinc are in opposite electrical states. And with regard to electricities of such very low intensity, water is an insulating body: every copper-plate consequently produces by induction an increase of positive electricity upon the opposite zinc plate; and every zinc plate an increase of negative electricity on the opposite copper-plate: and the intensity increases with the number, and the quantity with the extent of the series.

When a communication is made between the two extreme points, the opposite electricities tend to annihilate each other; and if the fluid medium could be a substance incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity cease. But solution of muriate of soda being composed of two series of elements possessing opposite electrical energies, the oxygene and the acid are attracted by the zinc, and the hydrogene and the alkali by the copper. The balance of power is momentary only; for solution of zinc is formed, and the hydro-

gene disengaged. The negative energy of the copper and the positive energy of the zinc are consequently again exerted, enfeebled only by the opposing energy of the soda in contact with the copper, and the process of electromotion continues, as long as the chemical changes are capable of being carried on.

This theory in some measure reconciles the hypothetical principles of the action of the pile adopted by its illustrious inventor, with the opinions concerning the chemical origin of GALVANISM, supported by the greater number of the British philosophers, and it is confirmed and strengthed by many facts and experiments.

Thus the VOLTAIC pile of 20 pairs of plates of copper and zinc exhibits no permanent electromotive power when the connecting fluid is water free from air;* for this substance does not readily undergo chemical change, and the equilibrium seems to be capable of being permanently restored through it. Concentrated sulphuric acid, which is a much more perfect conductor, is equally inefficient, for it has little action upon zinc, and is itself decomposed only by a very strong power. Piles, containing as their fluid element either pure water or sulphuric acid, will undoubtedly give single shocks, and this effect is connected with the restoration of the equilibrium disturbed by the energies of the metals; but when their extreme plates are connected there is no exhibition, as in usual cases of electromotion. Water containing loosely combined oxygene is more efficient than water containing common air, as it enables

* The experiments proving this fact, and the other analogous facts in this page, may be seen detailed in Nicholson's Journal, 4to. Vol. IV. page 338 and 394; and Phil. Mag. Vol. X. page 40.

oxide of zinc to be formed more rapidly, and in larger quantities. Neutrosaline solutions which are at first very active, loose their energy in proportion as their acid arranges itself on the side of the zinc, and their alkali on that of the copper; and I have found the powers of a combination nearly destroyed from this cause very much revived, merely by agitating the fluids in the cells and mixing their parts together. Diluted acids, which are themselves easily decomposed, or which assist the decomposition of water, are above all other substances powerful; for they dissolve the zinc, and furnish only a gaseous product to the negative surface, which is immediately disengaged.

There are other experiments connected with very striking results, which offer additional reasons for supposing the decomposition of the chemical menstrua essential to the continued electromotion in the pile.

As when an electrical discharge is produced by means of small metallic surfaces in the VOLTAIC battery, (the opposite states being exalted,) *sensible* heat is the consequence, it occurred to me, that if the decomposition of the chemical agents was essential to the balance of the opposed electricities, the effect, in a saline solution, of this decomposition, and of the transfer of the alkali to the negative side, and of the acid to the positive side, ought, under favourable circumstances, to be connected with an increase of temperature.

I placed the gold cones, which have been so often mentioned, in the circuit of the battery with the power of 100, I filled them with distilled water, and connected them by a piece of moistened asbestus, about an inch in length and $\frac{1}{6}$ of an inch diameter; I provided a small air-thermometer capable

of being immersed in the gold cones, expecting (if any) only a very slight change of temperature; I introduced a drop of solution of sulphate of potash into the positive cone: the decomposition instantly began: potash passed rapidly over into the negative cone, heat was immediately sensible; and in less than two minutes the water was in a state of ebullition.

I tried the same thing with a solution of nitrate of ammonia, and in this instance the heat rose to such an intensity as to evaporate all the water in three or four minutes, with a kind of explosive noise; and at last actual inflammation took place, with the decomposition and dissipation of the greatest part of the salt.*

That the increase of the conducting power of the water by the drop of saline solution had little or nothing to do with the effect, is evident from this circumstance. I introduced a quantity of strong lixivium of potash into the cones, and likewise concentrated sulphuric acid, separately, which are better conductors than solutions of the neutral salts; but there was very little sensible effect.

The same principles will apply to all the varieties of the electrical apparatus, whether containing double or single plates; and if the ideas developed in the preceding sections be correct, one property operating under different modifications is the universal cause of their activity.

* In this process ammonia was rapidly given off from the surface of the negative cone, and nitrous acid from that of the positive cone, and a white vapour was produced by their combination in the atmosphere above the apparatus.

X. *On some general Illustrations and Applications of the foregoing Facts and Principles, and Conclusion.*

The general ideas advanced in the preceding pages are evidently directly in contradiction to the opinion advanced by Fabroni, and which, in the early stage of the investigation, appeared extremely probable, namely, that chemical changes are the *primary* causes of the phænomena of Galvanism.

Before the experiments of M. Volta on the electricity excited by the mere contact of metals were published, I had to a certain extent adopted this opinion; but the new facts immediately proved that another power must necessarily be concerned; for it was not possible to refer the electricity exhibited by the apposition of metallic surfaces to any chemical alterations, particularly as the effect is more distinct in a dry atmosphere, in which even the most oxidable metals do not change, than in a moist one, in which many metals undergo chemical alteration.

Other facts likewise soon occurred demonstrative of the same thing. In the Voltaic combination of diluted nitrous acid, zinc and copper, as is well known, the side of the zinc exposed to the acid is positive. But in combinations of zinc, water and diluted nitric acid, the surface exposed to the acid is negative; though if the *chemical* action of the acid on the zinc had been the cause of the effect, it ought to be the same in both cases.

In mere cases of chemical change likewise electricity is never exhibited. Iron burnt in oxygene gas, properly connected with a condensing electrometer, gives no charge to it during the process. Nitre and charcoal deflagrated in com-

munication with the same instrument do not by their agencies in the slightest degree affect the gold leaves. Solid pure potash and sulphuric acid made to combine in an insulated platina crucible produce no electrical appearances. A solid amalgam of bismuth and a solid amalgam of lead become fluid when mixed together: the experiment, I find, is connected with a diminution of temperature, but with no exhibition of electrical effects. A thin plate of zinc, after being placed upon a surface of mercury, and separated by an insulating body, is found positive, the mercury is negative: the effects are exalted by heating the metals; but let them be kept in contact sufficiently long to amalgamate, and the compound gives no signs of electricity. I could mention a great number of other instances of *pure chemical* action in which I have used all the means in my power to ascertain the fact, and the result has been constantly the same. In cases of effervescence, indeed, particularly when accompanied by much heat, the metallic vessels employed become negative, but this is a phænomenon connected with *evaporation*, the change of state of a body independent of chemical change, and is to be referred to a different law.*

* The change of the capacities of bodies in consequence of the alteration in their volumes, or states of existence by heat, is a continually operating source of electrical effects: and as I have hinted, page 36, it often interferes with the results of experiments on the electrical energies of bodies as exhibited by contact. It is likewise probably one of the sources of the capricious results of experiments of friction, in which the same body, according as its texture is altered, or its temperature changed, assumes different states with regard to another body. Friction may be considered as a succession of contacts, and the natural energies of bodies would probably be accurately exhibited by it, if the unequal excitation of heat or its unequal communication to the different surfaces did not interfere by altering unequally their electrical capacities. Of the elements of flint glass, silex is slightly negative with

I mentioned the glass plates of BECCARIA as affording a parallel to the case of combination in consequence of the different electrical states of bodies. In GUYTON DE MORVEAU'S experiments on cohesion, the different metals are said to have adhered to mercury with a force proportional to their chemical affinities. But the other metals have different electrical energies, or different degrees of the same electrical energy with regard to this body; and in all cases of contact of mercury with another metal, upon a large surface, they ought to adhere in consequence of the difference of their electrical states, and that with a force proportional to the exaltation of those states. Iron, which M. GUYTON found slightly adhesive, I find exhibits little positive electricity after being laid upon a surface of mercury, and then separated. Tin, zinc, and copper, which adhere much more strongly, communicate higher charges to the condensing electrometer: I have had no instrument sufficiently exact to measure the differences: but it would seem that the adhesion from the difference of electrical states must have operated in these experiments,* which being proportional to the electrical energies are, on the hypothesis before stated, proportional to the chemical affinities. How far cohesion in general may be influenced or occasioned by this effect of the difference of the electrical energies of bodies is a curious question for investigation.

Many applications of the general facts and principles to

regard to the metals, the soda is positive; and in contacts of glass with metals I find it exhibits the excess of the energy of the alkali: the case, as is well known, is the same in friction, the amalgam of the common machine is essential to its powerful excitation.

* Amalgamation undoubtedly must have interfered; but the *genreal* result seems to have been distinct.

the processes of chemistry, both in art and in nature, will readily suggest themselves to the philosophical enquirer.

They offer very easy methods of separating acid and alkaline matter, when they exist in combination, either together or separately, in minerals; and the electrical powers of decomposition may be easily employed in animal and vegetable analysis.

A piece of muscular fibre, of two inches long and half an inch in diameter, after being electrified by the power of 150 for five days, became perfectly dry and hard, and left on incineration no saline matter. Potash, soda, ammonia, lime, and oxide of iron were evolved from it on the negative side, and the three common mineral acids and the phosphoric acid, were given out on the positive side.

A laurel leaf treated in the same manner, appeared as if it had been exposed to a heat of 500° or 600° FAHRENHEIT, and was brown and parched. Green colouring matter, with resin, alkali, and lime, appeared in the negative vessel: and the positive vessel contained a clear fluid, which had the smell of peach blossoms; and which, when neutralized by potash, gave a blue-green precipitate to solution of sulphate of iron; so that it contained vegetable prussic acid.

A small plant of mint, in a state of healthy vegetation, was made the medium of connection in the battery, its extremities being in contact with pure water; the process was carried on for 10 minutes: potash and lime were found in the negatively electrified water, and acid matter in the positively electrified water, which occasioned a precipitate in solutions of muriate of barytes, nitrate of silver, and muriate of lime. This plant recovered after the process: but a similar one, that had been

electrified for four hours with like results, faded and died.* The facts shew that the electrical powers of decomposition act even upon living vegetable matter; and there are some phænomena which seem to prove that they operate likewise upon living animal systems. When the fingers, after having been carefully washed with pure water, are brought in contact with this fluid in the positive part of the circuit, acid matter is rapidly developed, having the characters of a mixture of muriatic, phosphoric, and sulphuric acids: and if a similar trial be made in the negative part, fixed alkaline matter is as quickly exhibited.

The acid and alkaline tastes produced upon the tongue, in GALVANIC experiments, seem to depend upon the decomposition of the saline matter contained in the living animal substance, and perhaps in the saliva.

As acid and alkaline substances are capable of being separated from their combinations in living systems by electrical powers, there is every reason to believe that by converse methods they may be likewise introduced into the animal œconomy, or made to pass through the animal organs: and the same thing may be supposed of metallic oxides; and these ideas ought to lead to some new investigations in medicine and physiology.

It is not improbable that the electrical decomposition of the neutral salts in different cases may admit of œconomical uses.

* Seeds, I find, when placed in pure water in the positive part of the circuit, germinate much more rapidly than under common circumstances; but in the negative part of the circuit they do not germinate at all. Without supposing any peculiar effects from the different electricities which however *may* operate, the phænomenon may be accounted for from the saturation of the water near the positive metallic surface with oxygene, and of that near the negative surface with hydrogene.

Well burned charcoal and plumbago, or charcoal and iron, might be made the exciting powers; and such an arrangement if erected upon an extensive scale, neutrosaline matter being employed in every series, would, there is every reason to believe, produce large quantities of acids and alkalies with very little trouble or expence.

Ammonia and acids capable of decomposition, undergo chemical change in the VOLTAIC circuit only when they are in very concentrated solution, and in other cases are merely carried to their particular points of rest. This fact may induce us to hope that the new mode of analysis may lead us to the discovery of the *true* elements of bodies, if the materials acted on be employed in a certain state of concentration, and the electricity be sufficiently exalted. For if chemical union be of the nature which I have ventured to suppose, however strong the natural electrical energies of the elements of bodies may be, yet there is every probability of a limit to their strength: whereas the powers of our artificial instruments seem capable of indefinite increase.

Alterations of electrical equilibrium are continually taking place in nature; and it is probable that this influence, in its faculties of decomposition and transference, considerably interferes with the chemical alterations occurring in different parts of our system.

The electrical appearances which precede earthquakes and volcanic eruptions, and which have been described by the greater number of observers of these awful events, admit of very easy explanation on the principles that have been stated.

Besides the cases of sudden and violent change, there must be constant and tranquil alterations in which electricity is

concerned, produced in various parts of the interior strata of our globe.

Where pyritous strata and strata of coal-blende occur, where the pure metals or the sulphurets are found in contact with each other, or any conducting substances, and where different strata contain different saline menstrua, electricity must be continually manifested; and it is very probable, that many mineral formations have been materially influenced, or even occasioned by its agencies.

In an experiment that I made of electrifying a mixed solution of muriates of iron, of copper, of tin, and of cobalt, in a positive vessel, distilled water being in a negative vessel, all the four oxides passed along the asbestus, and into the negative tube, and a yellow metallic crust formed on the wire, and the oxides arranged themselves in a mixed state round the base of it.

In another experiment, in which carbonate of copper was diffused through water in a state of minute division, and a negative wire placed in a small perforated cube of zeolite in the water, green crystals collected round the cube; the particles not being capable of penetrating it.

By a multiplication of such instances the electrical power of transference may be easily conceived to apply to the explanation of some of the principal and most mysterious facts in geology.

And by imagining a scale of feeble powers, it would be easy to account for the association of the insoluble metallic and earthy compounds, containing acids.

Natural electricity has hitherto been little investigated,

except in the case of its evident and powerful concentration in the atmosphere.

Its slow and silent operations in every part of the surface will probably be found more immediately and importantly connected with the order and œconomy of nature; and investigations on this subject can hardly fail to enlighten our philosophical systems of the earth; and may possibly place new powers within our reach.

EXPLANATION OF THE FIGURES.

Plate I.

Fig. 1, Represents the agate cups, mentioned page 4.

Fig. 2, Represents the gold cones, page 6.

Fig. 3, Represents the glass tubes, and their attached apparatus, page 21.

Fig. 4, Represents the two glass tubes, with the intermediate vessel, page 22.

In all the figures A B denote the wires, rendered one positively, the other negatively electrical; and C the connecting pieces of moistened amianthus.

Fig. 1.

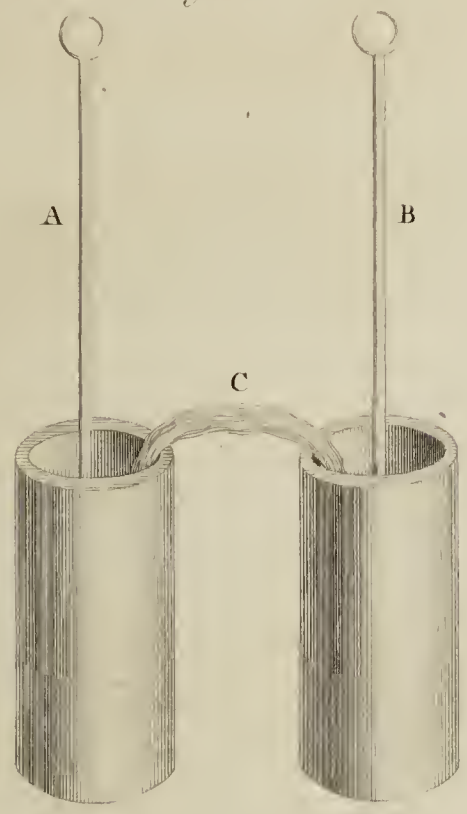


Fig. 2.

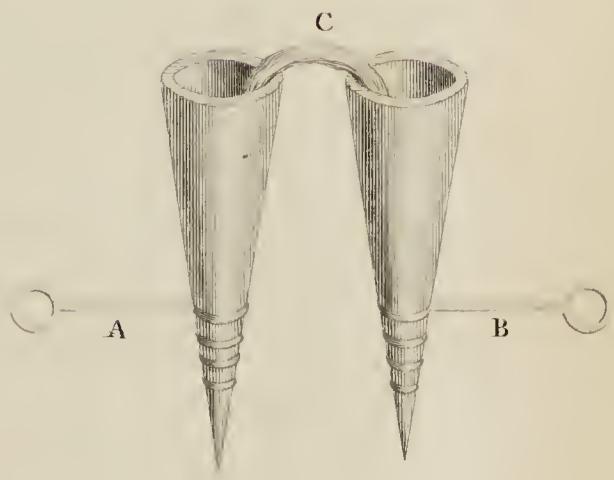


Fig. 3.

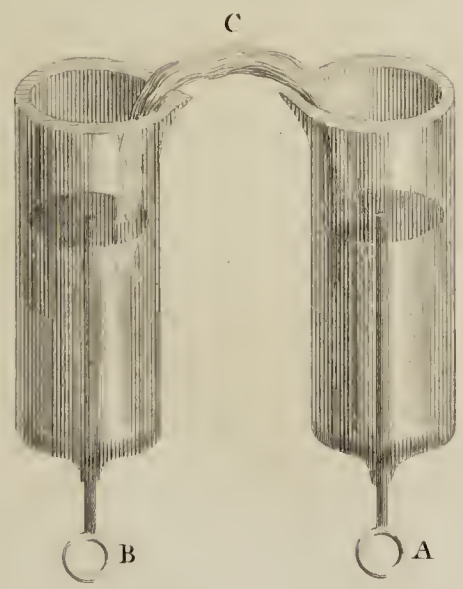
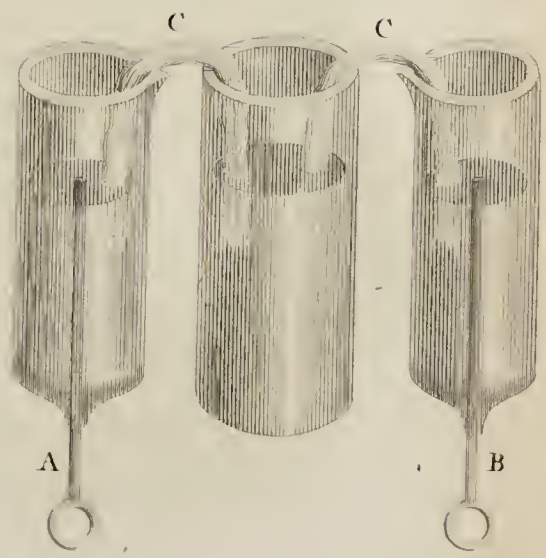


Fig. 4.



Basire sculp.

II. *On the Precession of the Equinoxes. By the Rev.* Abram Robertson, *M. A. F. R. S. Savilian Professor of Geometry in the University of Oxford.*

Read December 18, 1806.

PERHAPS the solution of no other problem, in natural philosophy, has so often baffled the attempts of mathematicians as that of determining the precession of the equinoxes, by the theory of gravity. The phenomenon itself was observed about one hundred and fifty years before the christian æra, but Sir ISAAC NEWTON was the first who endeavoured to estimate its magnitude by the true principles of motion, combined with the attractive influence of the sun and moon on the spheroidal figure of the earth. It has always been allowed, by those competent to judge, that his investigations relating to the subject evince the same transcendent abilities as are displayed in the other parts of his immortal work, THE MATHEMATICAL PRINCIPLES OF NATURAL PHILOSOPHY, but, for more than half a century past, it has been justly asserted that he made a mistake in his process, which rendered his conclusions erroneous.

Since the detection of this error, some of the most eminent mathematicians in Europe have attempted solutions of the problem. Their success has been various; but their investigations may be arranged under three general heads. Under the first of these may be placed such as lead to a wrong

conclusion, in consequence of a mistake committed in some part of the proceedings. The second head may be allotted to those in which the conclusions may be admitted as just, but rendered so by the counteraction of opposite errors. Such may be ranked under the third head as are conducted without error fatal to the conclusion, and in which the result is as near the truth as the subject seems to admit.

The authors of those investigations, of each of the three descriptions, are entitled to much praise. Their productions afford the most unquestionable proofs of great talents, great zeal, and great perseverance, exerted in the cultivation of science. The mistakes committed in those of the two first descriptions, and the obscurity and perplexity with which those of the third may be charged, are, in my opinion, to be attributed to the same cause, the uncultivated state of that particular department of the doctrine of motion, which constitutes the appropriate foundation for the solution of the problem. The department to which I allude is that of compound rotatory motion.

In consequence of this persuasion I have, in the first nine of the following articles, endeavoured to investigate the primary properties of compound rotatory motion from clear and unexceptionable principles. The disturbing solar force on the spheroidal figure of the earth is then calculated, and the angular velocity which it produces is afterwards compared with that of the diurnal revolution, by means of the properties of rotatory motion previously demonstrated. The quantity of annual precession is then calculated in the usual way, and also that of nutation, as far as they are produced by the disturbing force of the sun.

1. Let C, (Plate II. Fig. 1.) be the centre of two circular arcs AB, EF, which are the measures of the angles ACB, ECF; and let CB cut EF in D. Then, as the sectors ACB, ECD are similar, CA : CE :: AB : ED $= \frac{CE \times AB}{CA}$. But (Eu. 33. VI.) ED : EF :: the angle ACB : the angle ECF; and therefore $\frac{CE \times AB}{CA}$: EF :: < ACB : < ECF. Consequently CE×AB×<ECF $=$ CA × EF × < ACB; and therefore AB : EF :: CA × < ACB : CE × < ECF.

2. Let ACB, GEF (Fig. 2.) be two angles, of which the arcs AB, GF are the measures; and the radii CA, EG not being necessarily equal, let the sines BK, FQ be equal to one another. Let BH, FM be tangents to the curves; and let HD, MN be parallel to CA, EG respectively, and meet BK, FQ in D, N, as represented. Then as CBH, BDH are right angles, the triangles CBK, BHD are equiangular, and CB : BK :: BH : HD or its equal KL, if HL be drawn parallel to BK, and meet CA in L. Consequently BK $= \frac{CB \times KL}{BH}$. For the same reasons, if MP be parallel to FQ and meet EG in P, FQ $= \frac{EF \times QP}{FM}$; and therefore as BK, FQ are, by hypothesis, equal, $\frac{CB \times KL}{BH} = \frac{EF \times QP}{FM}$. Hence CB × KL × FM $=$ EF × QP × BH, and BH : FM :: CB × KL : EF × QP.

3. If, therefore, we suppose straight lines CH, EM to be drawn, and that the angles BCH, FEM are indefinitely small, and generated in the same time by the revolution of CB, EF respectively, then BH, FM may be considered as circular arcs, and by article 1, BH : FM :: CB × < BCH : EF × < FEM. Hence by article 2, (and 11. V.) CB × < BCH : EF × < FEM :: CB × KL : EF × QP, and therefore < BCH : < FEM :: KL

: QP. Consequently, during the generation of the angles BCH, FEM, the angular velocity of CB is to the angular velocity of EF as KL to QP.

4. If ACB (Fig. 3.) be any plane angle, and from any point E, EF be drawn perpendicular to AC and EH perpendicular to CB, then the angle FEH is equal to the angle ACB. For let HK be drawn perpendicular to AC, and let EH be produced to G. First let ACB be an obtuse angle, and then $< ACB = < KHC + < HKC + < KHC + < CHG = < KHG = (29.\text{I.}) < FEH$. Secondly, ACB being an acute angle, the right angle $GHC = < GHK + < KHC = < KHC + < HCK = (15.\text{I.}) < KHC + < ACB$. Consequently the angle $ACB = < GHK = < FEH$.

5. Let G (Fig. 4.) be the centre of gravity of a body, and AB, DC two axes passing through G; and, while the body revolves round AB, let AB and consequently the whole body revolve round DC, the periodical times of these revolutions not being necessarily equal; it is required to determine the direction and angular velocity with which any particle of the body revolves in consequence of this compound motion.

Suppose the simple motion of the body about AB to be such, that during the revolution the parts towards D from AB would rise above the plane, on which the figure is drawn, and the parts towards C sink below it. And suppose the simple motion about DC to be such, that during the revolution the parts towards A from DC would rise above the plane of the figure, and those towards B sink below it. Let P (Fig. 5.) be a particle of the body, above the plane, and let PR be a perpendicular to the said plane.* With the centre G suppose

* The axis DC, and the line RM are intentionally omitted in Fig. 5, with a view to prevent confusion in the figure.

a spherical superficies to be described, passing through P, and let ADBC be the great circle of this sphere in the plane on which the whole is represented. Let the straight lines EF, HK pass through R, and be perpendicular to AB, DC respectively; and let EPF, HPK be lesser circles of the sphere, EF being a diameter of the one, and HK a diameter of the other. Then it is evident, that by the simple motion of the body about AB only the particle P would move in the circumference FPE; and by the simple motion of the body about DC only, P would move in the circumference HPK. Let the indefinitely small arcs P*s*, P*q* be those which P would describe in equal times with the revolutions about AB, DC separately, and let the parallelogram P*qps* be completed on the spherical superficies. Then it is evident, from the composition of motion, that the direction and velocity of P, in consequence of the compound motion, is as the diagonal P*p* of the parallelogram P*qps*.

Let R*r*MN be the orthographical projection of P*qps* on the plane ADBC, and then as PR is perpendicular to this plane, it is evident that R*r*MN is a parallelogram, and that its diagonal RM is the direction and velocity of P in the projection, in consequence of the compound motion. It therefore follows, from article 3, as RN is the angular velocity about the axis AB, and R*r* that about the axis DC, that RM is the angular velocity about the axis, round which the body is caused to revolve by the compound motion.

6. The same things being supposed, and the parallelogram R*r*MN being the same in Fig. 4 as in Fig. 5, let RM produced meet the circumference in L and Q, and the diameter TGS, at right angles to LQ, is the axis sought.

The same axis may be obtained in the following manner.

In GB take GV equal to RN; and in GC take GW equal to R*r*, and VW being drawn it will be parallel to the axis TS. For as NR is perpendicular to AB, and R*r* or NM to DC, by article 4, the angle RNM, or (34. I.) its equal R*r*M, is equal to the angle VGW. Also, on account of the equals, VG : GW : : M*r* : *r*R, and therefore (6. VI.) the angles *r*RM, GWV are equal. Let TS meet HK in O, and LQ in I; and let DC meet HK in X. Then as the angle OIR is equal to the angle OXG, each being a right one, and as the angles IOR, XOG are equal, the angle IRO, or MR*r* is equal to the angle OGX. Consequently the alternate angles XGO, GWV are equal, and therefore TS, VW are parallel. Hence it is evident that if the axes AB, DC, and also GV, GW the angular velocities round them be given, the axis TS is easily found, being parallel to VW. It is proper to observe that GV, GW are to be set off on that side of TS towards which the body is moving, in consequence of the revolutions round DC, AB.

7. From the last article it is evident that VW is equal to RM, and consequently equal to the angular velocity, with which the body revolves about the axis TS. If therefore CGB be a right angle, then the angular velocity $VW = \sqrt{VG^2 + GW^2}$. In other cases the value of VW may be easily calculated by plane Trigonometry.

8. It is to be remarked, for the sake of precision, that the linear velocity, of any point, is as the angular velocity multiplied into the radius of the circle in whose circumference it revolves. Thus the linear velocity P*s* (Fig. 5.) of the point P in the circumference FPE, is as its angular velocity in the same, multiplied into the radius of the circle FPE, as is evi-

dent from article 3. In the following articles, linear velocity is meant when no adjective is annexed to the word velocity.

9. Let AB, DC (Fig. 6.) be the two axes about which separately the body would revolve, as stated in article 5, and let TS be the axis about which it revolves, in consequence of a combination of these two revolutions. Let TE be at right angles to AB, and meet it in H, and let TF be at right angles to DC and meet it in K; and let GV, GW be the angular velocities about AB, DC, as in the preceding articles. Then it follows, from the last article, that the velocity of the point T, by the revolution about the axis AB only, is equal to GV×HT. And as this velocity is in the direction of a tangent at T to the circle of which TE is a diameter, and as this circle is perpendicular to the plane ADBC, the direction of this velocity is evidently perpendicular to the plane ADBC. The direction of this velocity of the point T is also upwards from the plane of the figure, agreeable to the statement in article 5. Again, by the revolution about the axis DC only, the velocity of T is equal to GW × KT, and, for the foregoing reasons, the direction of this velocity of the point T is perpendicularly downwards below the plane, according to article 5. Now as TS is the axis about which the body revolves, in consequence of the combined revolutions about AB, DC, every point in TS is rendered quiescent by the compound motions. It is therefore evident that GV×HT=GW×KT.

10. The revolutions about DC, AB may be supposed to be caused by instantaneous impulses at A and D, made at the same time, or at different times; or they may be supposed to be occasioned by the agency of constant forces, like that of

gravity. For if the causes be adequate to the production of the same velocities, taken separately, and in the same directions, the velocity and direction of a particle will be the same from their combined influence upon it, whether these causes be impulses or constant forces.

As the body is understood to be in free space, if the causes of the revolutions, taken separately, be instantaneous impulses, and made at the same time, immediately after their agency the body will revolve about the axis TS, and it will continue so to revolve with an uniform velocity. If whilst the body is revolving with an uniform velocity about the axis DC, a constant force begin to act at D, so as to cause a tendency to revolution about AB, as stated in article 5, and continue afterwards to act at T, the pole of the new axis, from a combination of the constant agency at the new pole and the uniform velocity about DC, the axis TS will incessantly shift its position.

Such exactly are the circumstances to which the earth is subject as to the production of the precession of the equinoxes. At the vernal equinox, for instance, a straight line drawn from the centre of the sun to that of the earth is in the plane of the equator, and therefore, as equal portions of the protuberant matter of the earth are above and below the ecliptic, the attractive power of the sun has no tendency to alter the position of the equator. But, in consequence of the earth's motion in its orbit, it very soon after the equinox presents a different position of the equator to the sun. The equilibrium of the protuberant parts of the earth, above and below the ecliptic, and towards the sun, is then done away,

and the attraction of the sun on that side, where the greatest quantity of protuberant matter is, tends to bring down the equator into the ecliptic, or to cause the earth to revolve about a diameter of the equator. This attractive influence of the sun gradually increases a little till the summer solstice; it then gradually decreases in the same degree till the autumnal equinox, when it vanishes. From the autumnal equinox to the winter solstice it again gradually increases a little; and it then gradually decreases in the same degree till the vernal equinox, when it again vanishes. This recurrence and continuance of action is annually repeated.

Similar observations apply to the attraction of the moon on the protuberant parts of the earth. When a straight line drawn from her centre to that of the earth is in the plane of the equator, the attractive influence of the moon has no tendency to change the position of the equator, but in other situations, the attraction of the moon tends to bring the equator of the earth into the plane of the moon's orbit, or causes the earth to move round a diameter of the equator. The recurrences of the moon's action on the protuberant parts of the earth, and the times of their continuing, are repeated every month.

These effects of the sun and moon are to be considered separately; and for the reasons already stated, each of the actions, combined with the diurnal revolution of the earth, may be considered as a particular case of compound rotatory motion. It is needless, however, after investigating the effects of the sun's action, and expressing them in general formulæ, to go over the same steps for ascertaining those of the moon,

as they may be inferred from the former, after making due allowance for the different circumstances under which these two remote bodies act on the protuberant parts of the earth.

I now proceed to estimate the force with which the sun tends to cause the earth to revolve about a diameter of the equator.

11. Let S be the centre of the sun (Fig. 7.) C that of the earth; P, L the poles, PL the axis; and let a plane passing through SC, PL cut the earth in the meridian PEAQB. Let EQ be the diameter of the equator, and let DF, the diameter of the spheroid in the plane SPCL, be at right angles to SC. Let SC cut the meridian EPQL in A, B; and G being supposed a particle of matter in this meridian, let GH parallel to SC meet DF in H, and let SG be drawn. Let M be the quantity of matter in the sun, or its absolute attracting power, and then $\frac{M}{SG^2}$ is its force upon the particle G, in the direction SG, and $\frac{M}{SC^2}$ is its force upon a particle at C, in the direction SC. But a force whose power and direction is as GS is equal to a force whose power and direction is as GC, together with a force whose power and direction is as CS; and as the force whose power and direction is as GC, is directed to the centre it has no tendency to alter the position of the axis PL, and therefore may be neglected in the present enquiry. Now, by Mechanics, $SG : SC :: \frac{M}{SG^2} : \frac{M \times SC}{SG^3}$ = the force of the sun on the particle G, in the direction CS or HG. Now as the distance of the sun from the earth is indefinitely great when compared to the diameter DF, its force on any particle in DF is equal to its force on a particle at C, and therefore the sun's force on a particle at H is as $\frac{M}{SC^2}$. Consequently, as the sun's

force on the particle G, in the same direction, is as $\frac{M \times SC}{SG^3}$, the disturbing force of the sun, by its action on the particle G is $\frac{M \times SC}{SG^3} - \frac{M}{SC^2}$, or $\frac{M \times SC}{|SC - GH|^3} - \frac{M}{SC^2}$, for SG may be considered as equal to SC — GH. But as SC is indefinitely great with respect to GH, $\frac{SC}{|SC - GH|^3}$, by actual division may be considered as equal to $\frac{1}{SC^2} + \frac{3GH}{SC^3}$, and therefore the disturbing force on the particle at G is $\frac{3M \times GH}{SC^3}$.

Let K be a particle in the meridian, but on the opposite side of DF to that on which G is situated. Let KN, parallel to SC, meet DF in N; and suppose SK to be drawn. Then the force of the sun on K being $\frac{M}{SK^2}$, for the same reasons as before, its force upon it in the direction of SC or KN is $\frac{M \times SC}{|SC + KN|^3}$, and after a reduction similar to the foregoing, the sun's disturbing force on K is $-\frac{3M \times KN}{SC^3}$.

Hence it is evident, supposing M and SC to be constant, that the disturbing force of the sun on any particle in the meridian PELQ is as the distance of the particle from DF; and that the sign of the force in the half DAF nearest to S is positive, but the sign of the force in the other half DBF is negative. This difference of the signs indicates that the particles on the opposite sides of DF have a directly opposite tendency, as to direction, in affecting the position of the axis PL, or equator EQ; and the same is evident from the following considerations. As the disturbing force is as its distance from DF, it has no effect on particles in DF, and therefore the inertia * of

* By this expression that part of the inertia is meant which opposes the disturbing force of the sun; and the same is to be understood in the following expressions.

particles in DF may be considered as equal to the sun's disturbing force, on the principle of action and reaction being equal as to magnitude, but directly contrary as to direction. But on the side of DF nearest to S the disturbing force is greater than the inertia of any particle G, and it therefore urges the particle from DF towards S, by a pressure whose direction and power is as HG. On the side of DF opposite to S the disturbing force is less than the inertia of any particle K, and therefore the inertia of K opposes the disturbing force of the sun by a pressure whose direction is from N towards K, and whose power is as NK.

12. As, by the nature of the spheroid, PELQ is an ellipse, let GK be the diameter conjugate to DF, and let VI, parallel to DF, meet it in T, and AB in R; and then VI is bisected in T. Let RI be bisected in v, and let w, q be two points in RI, equally distant from R and I respectively. Let $a = \text{EC}$, and $d =$ the disturbing force of the sun at the distance of EC from DF. Then by the preceding article, $a : \text{RC} :: d : \frac{d}{a} \times \text{RC}$ = the force at R, or at any point in VI, as any two points in VI are equally distant from DF. Now it is evident that the disturbing force on a particle at R, or on any particle in AC, has no power to turn the ellipse about C; but the force on a particle at w tends to turn the ellipse about the centre, for it is applied at the end of the lever Rw. Consequently, by what has been already proved in this article, and by the property of the lever, the force on w to turn the ellipse is $\frac{d}{a} \times \text{RC} \times \text{R}w$. For the same reasons, the force on q to turn the ellipse is $\frac{d}{a} \times \text{RC} \times \text{R}q$, and therefore the force on w and q combined, to turn the ellipse, is $\frac{d}{a} \times \text{RC} \times \text{RI}$, for $\text{R}w + \text{R}q = \text{RI}$.

Hence, as Rv or $\frac{RI}{2}$ expresses the number of particles in Rv or vI, it follows that the force of all the particles in RI, to turn the ellipse, is $\frac{d}{a} \times RC \times RI \times \frac{RI}{2} = \frac{d}{2a} \times RC \times RI^2$. In the same way it may be proved that the force of all the particles in VR, to turn the ellipse, is $\frac{d}{2a} \times RC \times RV^2$. But the force of the particles in RI tends to turn the ellipse upwards in the direction FAD, and the force of the particles in RV tends to turn it downwards in the contrary direction DAF. The force of all the particles in VI, therefore, to turn the ellipse, is $\frac{d}{2a} \times RC \times \overline{RV^2 - RI^2}$. But as TV is half the sum of RV, RI, it follows that RT is half their difference, and therefore $RV^2 - RI^2 = VI \times 2RT$. Consequently the force of the particles in VI, to turn the ellipse, is $\frac{d}{2a} \times RC \times VI \times 2RT = \frac{d}{a} \times RC \times VI \times RT$.

13. Let $c = CG$, $b = CD$, $f = GH$, $g = CH$, $y = CR$, and $x = CT$. Then by similar triangles, $c : f :: x : y = \frac{fx}{c}$; and $c : g :: x : \frac{gx}{c} = RT$. Also, by the property of the ellipse, $c^2 : b^2 :: GT \times TK : TV^2 :: \overline{c+x} \times \overline{c-x} : TV^2 :: c^2 - x^2 : TV^2$. Consequently $TV = \frac{b}{c}\sqrt{c^2 - x^2}$, and $VI = \frac{2b}{c}\sqrt{c^2 - x^2}$, and the force of the particles in VI, to turn the ellipse, is $\frac{d}{a} \times y \times \frac{2b}{c}\sqrt{c^2 - x^2} \times \frac{gx}{c} = \frac{2bdfgx^2}{ac^3}\sqrt{c^2 - x^2}$, by putting $\frac{fx}{c}$ for y. The fluxion of this force is therefore $\frac{2bdfgx^2}{ac^3}\sqrt{c^2 - x^2} \times \dot{y} = \frac{2bdfgx^2}{ac^3}\sqrt{c^2 - x^2} \times \frac{f\dot{x}}{c} = \frac{2bdffgx^2\dot{x}}{ac^4}\sqrt{c^2 - x^2}$, as $\frac{f\dot{x}}{c} = \dot{y}$. The fluent of this expression may be found in the following manner.

14. The fluxion of $x \times \overline{c^2 - x^2}^{\frac{3}{2}}$ is $\dot{x} \times \overline{c^2 - x^2}^{\frac{3}{2}} + \frac{3}{2} \times x \times \overline{c^2 - x^2}^{\frac{1}{2}} \times -2x\dot{x} = \dot{x} \times \overline{c^2 - x^2}^{\frac{3}{2}} - 3\sqrt{c^2 - x^2} \times x^2\dot{x} = \dot{x} \times \overline{c^2 - x^2}$

$\times \sqrt{c^2-x^2} - 3\sqrt{c^2-x^2} \times x^2\ \dot{x} = \overline{c^2\ \dot{x} - x^2\ \dot{x}} \times \sqrt{c^2-x^2} - 3\sqrt{c^2-x^2} \times x^2\ \dot{x} = c^2\ \dot{x}\ \sqrt{c^2-x^2} - 4x^2\ \dot{x}\ \sqrt{c^2-x^2}$. Consequently the fluxion of $\frac{x \times \overline{c^2-x^2}|^{\frac{3}{2}}}{4}$ is $\dot{x}\ \sqrt{c^2-x^2} \times \frac{c^2}{4} - x^2\ \dot{x}\ \sqrt{c^2-x^2}$; and the fluxion of $\frac{2bf}{c^2} \times \frac{x \times \overline{c^2-x^2}|^{\frac{3}{2}}}{4}$ is $\frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2} \times \frac{c^2}{4} - \frac{2bf}{c^2} \times x^2\ \dot{x}\ \sqrt{c^2-x^2}$. Hence the fluent of $\frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2} \times \frac{c^2}{4} - \frac{2bf}{c^2} \times x^2\ \dot{x}\sqrt{c^2-x^2}$ = the fluent of $\frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2} \times \frac{c^2}{4}$ — the fluent of $\frac{2bf}{c^2} \times x^2\ \dot{x}\ \sqrt{c^2-x^2} = \frac{2bf}{c^2} \times \frac{x \times \overline{c^2-x^2}|^{\frac{3}{2}}}{4}$; and therefore by transposition, the fluent of $\frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2} \times \frac{c^2}{4} - \frac{2bf}{c^2} \times \frac{x \times \overline{c^2-x^2}|^{\frac{3}{2}}}{4}$ = the fluent of $\frac{2bf}{c^2} \times x^2\ \dot{x}\ \sqrt{c^2-x^2}$. Again, as VI is equal to $\frac{2b}{c}\sqrt{c^2-x^2}$, the fluxion of the area DVIF is $\frac{2b}{c}\sqrt{c^2-x^2} \times \dot{y} = \frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2}$, and therefore the fluent of $\frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2}$ is the area DVIF; and the fluent of $\frac{2bf}{c^2} \times \dot{x}\ \sqrt{c^2-x^2} \times \frac{c^2}{4}$ is the area DVIF $\times \frac{c^2}{4}$. Consequently the area DVIF $\times \frac{c^2}{4} - \frac{2bf}{c^2} \times \frac{x \times \overline{c^2-x^2}|^{\frac{3}{2}}}{4}$ = the fluent of $\frac{2bf}{c^2} \times x^2\ \dot{x}\ \sqrt{c^2-x^2}$, and therefore $\frac{dfg}{ac^2} \times$ area DVIF $\times \frac{c^2}{4} - \frac{dfg}{ac^2} \times \frac{2bf}{c^2} \times \frac{x \times \overline{c^2-x^2}|^{\frac{3}{2}}}{4}$ = the fluent of $\frac{dfg}{ac^2} \times \frac{2bf}{c^2} \times x^2\ \dot{x}\ \sqrt{c^2-x^2}$, or the force of the particles in the area DVIF to turn the ellipse. Hence, when x becomes equal to c, the force of all the particles in the semi-ellipse DGF, to turn the ellipse, is equal to the semi-ellipse DGF $\times \frac{dfg}{ac^2} \times \frac{c^2}{4}$ = the semi-ellipse DGF $\times \frac{d}{a} \times \frac{f}{4} \times g$. By article 11, therefore, the force of all the particles in the whole ellipse, which tend to turn it about the centre, is the whole ellipse PELQ $\times \frac{d}{a} \times \frac{f}{4} \times g$.

15. The other circumstances as to the figure being the same as before, let the straight line GV (Fig. 8.) touch the ellipse in G, meet QE in V and CM parallel to GH in M. Let GI be perpendicular to QE and meet it in I, and let GH meet EQ in T. Then, by a well known property of the ellipse, CI : IT : : EQ : its parameter; and therefore by the nature of a parmeter, or three proportionals, CI : IT : : CE^2 : CP^2, and $IT = \frac{CI \times CP^2}{CE^2}$. Hence $CT = CI - IT = CI - \frac{CI \times CP^2}{CE^2}$; and by another well known property of the ellipse, $CV = \frac{CE^2}{CI}$. Consequently $CT \times CV = CE^2 - CP^2$. Now as MAC is part of the straight line drawn from the centre of the sun to that of the earth, the angle ACE is the sun's declination, and as DC is perpendicular to AC, the angle ECD is the complement of the declination. Let $m =$ the sine of the declination, and $n =$ its cosine, and then, radius being 1, CT : CH : : 1 : m, and CV : CM or (34. I.) its equal GH : : 1 : n. By multiplication therefore, CT × CV : CH × GH : : 1 : mn. Consequenty, using the same notation as in the last article, and putting $e =$ CP, as $CT \times CV = CE^2 - CP^2$, we have $a^2 - e^2 : fg : : 1 : mn$, and $fg = \sqrt{a^2 - e^2} \times mn$. If therefore X denote the area of the ellipse PELQ, by the last article, the force of all the particles to turn the ellipse is $\frac{d}{a} \times \frac{mn}{4} \times \sqrt{a^2 - e^2} \times X$.

16. Let PELQ be the same ellipse in Fig. 9, as in the 7th and 8th figures. Let the spheroid be cut by a plane parallel to PELQ, (Fig. 9,) and let the section be the ellipse HKNG; and let this ellipse be supposed to be above the plane of the paper on which PELQ is represented. Again, let the spheroid be cut by a plane passing through PL, and perpendicular to the plane PELQ, and let the line of common section of this

plane with the ellipse HKNG be HN, and let this ellipse be cut by the plane of the equator in the line KG. Then HN (14. XI.) is parallel to PL, and KG to EQ; and therefore (10. XI.) HN, KG cut one another at right angles. Again, as the axis PL is perpendicular to the plane of the equator, the plane of the equator is perpendicular (18. XI.) to the ellipse PELQ. The planes passing through the centre of the spheroid and the lines HN, KG are therefore perpendicular to the ellipse PELQ, and consequently (19. XI.) the straight line passing through the centre of the spheroid, and the point in which HN, KG cut one another, is perpendicular to HN, KG and also to PL, EQ. Consequently as the equator is a circle, KG (3. III.) is bisected by HN; and as HN is parallel to PL it is a double ordinate to the diameter of the equator passing through the point in which HN, KG cut one another, and is therefore bisected in this point. Hence, as by a well known property of the spheroid, the ellipses PELQ, HKNG are similar, it is evident that KG is the transverse, and HN the conjugate axis of the ellipse HKNG.

Let u = the distance of the centre of the ellipse HKNG from the centre of the speroid, and then as the points E, K, G, Q are in the circumference of the equator, a straight line drawn from the centre of the spheroid to K or G is equal to a, and half of the straight line KG $= \sqrt{a^2-u^2}$. Again, as the ellipses PELQ, HKNG are similar, $a : e :: \sqrt{a^2-u^2} : \frac{e}{a}\sqrt{a^2-u^2}$ = half of HN; and $a^2 : a^2-u^2 :: X : \frac{a^2-u^2}{a^2} \times X$ = the area of the ellipse HKNG. Hence, in order to find the disturbing force of the sun on the ellipse HKNG, instead of a^2 in the expression $\frac{d}{a} \times \frac{mn}{4} \times \overline{a^2-e^2} \times X$ we are to put a^2-u^2, instead of

e^2 we are to put $\frac{e^2}{a^2} \times \overline{a^2 - u^2}$, and instead of X we are to put $\frac{a^2-u^2}{a^2}$ X. These substitutions being made, we have $\frac{d}{a} \times \frac{mn}{4} \times \frac{a^2-e^2}{a^2} \times \overline{a^2-u^2} \times \frac{a^2-u^2}{a^2}$ X for the sun's disturbing force on the ellipse HKNG, tending to turn the ellipse about an axis passing through C and perpendicular to the plane PELQ. This expression for the force being multiplied by $\dot{u}$, gives the fluxion of the force on that part of the spheroid between the ellipses PELQ, HKNG; and the fluent of this, when u becomes equal to a is $\frac{d}{a} \times \frac{mn}{4} \times \frac{a^2-e^2}{a^2} \times \frac{8a^3}{15}$ X. Consequently the double of this, *viz.* $\frac{d}{a} \times mn \times \overline{a^2-e^2} \times \frac{4a}{15}$ X expresses the sun's disturbing force on the whole spheroid. Hence if $Z = \frac{4a}{3}X$, which expresses the solid content of the spheroid, the force on the whole spheroid is $\frac{d}{a} \times \frac{mn}{5} \times \overline{a^2-e^2} \times Z$. Let this be called the librating force or pressure, or the force causing libration.

17. It is evident, from the manner in which the librating pressure is calculated, that the whole of the disturbing force is occasioned by the protuberance of the spheroid above the greatest inscribed sphere. For if PELQ were a sphere, as VI (Fig. 7.) is parallel to the diameter DF, and AC perpendicular to it, the straight line VI (3. III.) would be bisected in R; and therefore the disturbing forces, above and below AC would exactly counteract one another.

Let DCF (Fig. 9.) denote a plane perpendicular to the straight line SC, then it is evident that the librating pressure tends to move the earth about that diameter of the equator, which is the common section of the equator and the plane DCF. For the sake of precision hereafter let this diameter of the equator be called the axis of libration.

The point E of the equator, nearest the sun, is at the distance of a quadrant from either extremity of the axis of libration. For, by hypothesis, SC is at right angles to the plane DCF, and therefore the axis of libration, which is in this plane, is at right angles to SC. Again, as PC the axis of the earth is at right angles to the plane of the equator, the axis of libration, which is also in the equator, is at right angles to CP. The axis of libration, therefore, being at right angles to CS, CP in the plane PELQ, is at right angles to CE in the same plane.

18. Let ADBE (Fig. 10.) be an oblate spheroid of which AB is the transverse axis, DE the conjugate axis, and C the centre. Let AKMBLH be the equator of this spheroid, and consequently at right angles to ADBE the generating ellipse. Let the spheroid be cut through DCE by a plane DMEL at right angles to ADBE, and let MCL be its common section with the equator. Then (19. XI.) MCL is at right angles to ADBE, and therefore ACM is a right angle; and as ACD is a right angle, AC is at right angles to the plane DMEL, and consequently at right angles to any plane parallel to it. Let the spheroid be cut by a plane parallel to DMEL, and let the common section of this plane with the spheroid be the ellipse FKGH. Let this ellipse cut the plane ADBE in the straight line F*r*G, and the plane of the equator in K*r*H, the point *r* being the centre of this last formed ellipse, or that point in which it meets AB. Then, by a well known property of the spheroid, the ellipses DMEL, FKGH are similar, and the area of the first is to that of the other as CA^2 or its equal CM^2 to rK^2. Put $a = AC$ or CM, $e = DC$, $x = Cr$, and the force of each particle in the spheroid being as its distance from the

plane DMEL, let v be the force of a particle at A. Let $p =$ the area of a circle whose diameter is 1. Then $4pae =$ the area of the ellipse DMEL, and as $\overline{a+x} \times \overline{a-x} = a^2-x^2 = rK^2$, $a^2 : a^2-x^2 :: 4pae : \frac{4pe}{a} \times \overline{a^2-x^2} =$ the area of the ellipse FKGH. Again, $a : x :: v : \frac{vx}{a} =$ the force of a particle at r, and therefore $\frac{4pev}{a^2} \times \overline{a^2\,x-x^3} =$ the force of all the particles in the ellipse FKGH. Now as this force acts at r, by the property of the lever, the power of the ellipse FKGH, to turn the spheroid, either about DE or ML as an axis, is $\frac{4pev}{a^2} \times \overline{a^2\,x^2-x^4}$; and the fluxion of this force is $\frac{4pev}{a^2} \times \overline{a^2\,x^2\,\dot{x}-x^4\,\dot{x}}$. The fluent of this, when x becomes equal to a, is $\frac{8pa^3\,ev}{15}$; and the double of this, for the force of the whole spheriod, is $\frac{16pa^3\,ev}{15}$. Hence it is evident that if the force of each particle in the spheroid, to cause a revolution, be as its distance from the plane DMEL, the particles on one side of this plane having a tendency to cause a revolution in one direction, and the particles on the other side of the plane having an equal tendency to cause a revolution in the same direction, then the pressure with which the spheroid is urged to revolve, either about DE or ML, is as $\frac{16pa^3\,ev}{15}$. This force is equal to $\frac{av}{5}$ Z, if Z be put equal to $\frac{16pa^2\,e}{3}$, the solid content of the spheroid.

19. As the librating force $\frac{d}{a} \times \frac{mn}{5} \times \overline{a^2-e^2} \times$ Z, ascertained in article 16, and the force $\frac{av}{5}$ Z, obtained in the last article, are calculated on the same hypothesis, *viz.* that the force of a particle is as its distance from the plane DCF in Fig. 9, or the plane DMEL in Fig. 10, if they produce equal angular velocities, the spheroids in the two figures being equal in every

respect, and all other circumstances being the same, the forces themselves must be equal. Now at either of the equinoxes the other circumstances are exactly the same in the two figures. At the vernal equinox, for instance, the straight line SACB in Fig. 9. must be in the plane of the equator, and therefore the plane DCF, perpendicular to AB, at this time must pass through the poles P, L. At the equinox, therefore, the straight line SACB, and the plane DCF in Fig. 9. are justly represented by ACB, and DMEL in Fig. 10. Hence the librating force $\frac{d}{a} \times \frac{mn}{5} \times \overline{a^2 - e^2} \times Z$ at the commencement of its action, at the equinox, applies to Fig. 10, and at its commencement it is equally efficacious to cause revolution about DE or ML. We are therefore enabled to compare the effect of the librating force, or the revolution it is capable of producing, at the equinox, about ML, with the diurnal revolution of the earth about DE, in the following manner.

It being admitted that each of the two forces, stated in the beginning of the article, produces the same angular velocity, then $\frac{d}{a} \times \frac{mn}{5} \times \overline{a^2 - e^2} \times Z = \frac{av}{5} Z$, and therefore $d \times mn \times \frac{a^2 - e^2}{a^2} = v$. But if a constant force act for a given time t, and cause the body to move on which it acts, the velocity generated from the commencement of the motion is as the force. Consequently, as v denotes the force acting on a particle at A, during the given time t, and as the forces acting on the other particles of the spheroid are proportional to their distances from the plane DMEL; the angular velocity of A, acquired in the given time t, is also accurately expressed by v. If therefore the force $\frac{av}{5} Z$ cease to act at the end of the given time t, the point A, as the spheroid is in free space, will afterwards revolve

with the uniform angular velocity v; but by the doctrine of constant forces, the angle described by A, during the action of $\frac{av}{5}$ Z is equal to $\frac{v}{2} = \frac{d}{2} \times mn \times \frac{a^2 - e^2}{a^2}$.

20. Let AB (Fig. 6.) represent that diameter of the equator about which the librating force begins to cause revolution at the equinox. Let G be the centre of the earth, and in GB let GV be taken equal to $\frac{v}{2}$, or $\frac{d}{2} \times mn \times \frac{a^2 - e^2}{a^2}$. Let w denote the angular diurnal velocity of the earth about its axis DC: and in GC let GW be taken equal to w. The points V, W being joined, let TGS be drawn parallel to VW, and by article 6, TS is the axis about which the earth will now revolve, in consequence of the diurnal revolution being combined with the libration about AB. From T, a pole of this axis, let TK be drawn perpendicular to DG. Then, by article 9, GW : GV :: GK : KT. But as the angle DGT is extremely small, GK may be considered as equal to the radius, and the arc DT as equal to its sine. Consequently, using the notation already specified, and considering a as radius, $w : \frac{d}{2} \times mn \times \frac{a^2 - e^2}{a^2} :: a : \frac{ad}{2w} \times mn \times \frac{a^2 - e^2}{a^2}$ = the angular velocity caused by the librating force. Our next object is to find the value of d in known terms.

21. If t be put for the time of the earth's diurnal revolution round its axis, and T be put for the earth's annual revolution round the sun, then $\frac{w^2}{a}$ is equal to the centripetal force on a body revolving at the equator in the time t, with the velocity w; and, using the notation of article 11, $\frac{M}{SC^2}$ is equal to the centripetal force of the sun on the earth. By the doctrine of centripetal forces therefore $\frac{M}{SC^2} : \frac{w^2}{a} :: \frac{SC}{T^2} : \frac{a}{t^2}$, and $\frac{a \times M}{SC^2 \times t^2} =$

$\frac{w^2 \times SC}{a \times T^2}$, and $M = \frac{w^2 \times SC^3 \times t^2}{a^2 \times T^2}$. But, by article 11, the disturbing force of the sun on a particle at G (Fig. 7.) is equal to $\frac{3M \times GH}{SC^3}$; and at the distance a from DF the disturbing force is $\frac{3M \times a}{SC^3}$. The foregoing value of M being substituted for it in this expression, we have $\frac{3w^2 \times t^2}{a \times T^2} = d$, the sun's disturbing force at the distance a from DF.

This value of d being put for it in the expression at the end of the last article, it follows that the angular velocity of libration, at its commencement at the equinox, is to the uniform angular diurnal velocity as $\frac{3w \times t^2}{2T^2} \times mn \times \frac{a^2 - e^2}{a^2}$ to w, or as $\frac{3t^2}{2T^2} \times mn \times \frac{a^2 - e^2}{a^2}$ to 1. But, according to the preceding notation, $t : \dot{t} :: 360° : \frac{360\dot{t}}{t}$ = the uniform angular diurnal velocity, and therefore $1 : \frac{3t^2}{2T^2} \times mn \times \frac{a^2 - e^2}{a^2} :: \frac{360\dot{t}}{t} : 360 \times \frac{3t\dot{t}}{2T^2} \times mn \times \frac{a^2 - e^2}{a^2}$ = the angular velocity of libration, at its commencement at the equinox. But as the product mn is the only variable quantity which enters into the value of the librating force, obtained in article 16, it is evident that $360 \times \frac{3t\dot{t}}{2T^2} \times mn \times \frac{a^2 - e^2}{a^2}$ expresses the momentary angular velocity of libration at any time. We are now to consider this effect of the librating force in the direction in which the force is exerted, *viz.* in a meridian analogous to PEAQ in Figure 7.

22. Let FLGA (Fig. 11.) represent the ecliptic on the sphere, S the sun's place in it, L the first point of Libra and A that of Aries; LBA the position of the equator when the sun is at S, and SB the sun's declination. Let FBG be the position into which the equator is pressed in the time $\dot{t}$, by a combination of the librating force and the diurnal

revolution; or, which comes to the same, let the spherical angle FBL or ABG be equal to $360 \times \frac{3t\dot{t}}{2T^2} \times mn \times \frac{a^2-e^2}{a^2}$. Let $h = \frac{a^2-e^2}{a^2}$. Then by spherical Trigonometry, sin. F : sin. BL : : sin. B : sin. FL; and therefore, as FL and the angle FBL are extremely small, the momentary precession $FL = 360 \times \frac{3t\dot{t}}{2T^2} \times hmn \times \frac{\text{sin. BL}}{\text{sin. F}}$.

23. If FG be bisected in C, and the arc CE be perpendicular to FG, meeting LBA in E, and FBG in D; then the arc CD is the measure of the angle at F. Also, as FL is extremely small, CE may be taken for the measure of the angle at L. Hence as BDE is a right angle, radius : sin. BE or cos. BL : : sin. EBD : sin. ED. Consequently as ED is extremely small, $360 \times \frac{3t\dot{t}}{2T^2} \times hmn \times \frac{\text{cos. BL}}{\text{radius}} = \text{ED}$, the momentary nutation, or the momentary change in the inclination of the equator to the ecliptic.

From the last proportion, and that concluding the preceding article, it follows that $FL : ED : : \frac{\text{sin. BL}}{\text{sin. F}} : \frac{\text{cos. BL}}{\text{radius}} : : \frac{\text{radius} \times \text{sin. BL}}{\text{cos. BL}}$: sin. F. But cos. BL : radius : : sin. BL : $\frac{\text{radius} \times \text{sin. BL}}{\text{cos. BL}} = \text{tang. BL}$; and therefore, the momentary precession FL is to the momentary nutation ED, as the tangent of the right ascension BL to the sine of the obliquity of the ecliptic.

24. Let b = the sine of the obliquity of the ecliptic, c = its cosine; z = the arc LS, x = its sine, and y = its cosine; and let $2p$ = the circumference of the ecliptic. Then as LBS is a right angle, by the circular parts, cos. BS × cos. BL = radius × cos. LS; that is $n \times$ cos. BL $= y$, radius being 1. Again, radius : x : : b : bx = the sine of BS = m. Consequently, cos. BS × sin. BS × cos. BL = $mn \times$ cos. BL = bxy; and therefore,

by the preceding article, the momentary nutation is $360 \times \frac{3t\dot{t}}{2T^2} \times hbxy$, radius being 1.

Again, $2p : \dot{z} :: T : \frac{T\dot{z}}{2p} = \dot{t}$. Also $\sqrt{1-xx} = y$, and, by the fluxional doctrine of circular arcs, $\dot{z} = \frac{\dot{x}}{\sqrt{1-xx}}$; and therefore $\dot{t} = \frac{T\dot{x}}{2p\sqrt{1-xx}}$. These values of $\dot{t}$ and y being put for them in the above expression, the momentary nutation, or, which is the same thing, the fluxion of the nutation is $360 \times \frac{3t}{2T} \times \frac{bbx\dot{x}}{2p}$. Consequently the nutation, when the sun is at S, is $360 \times \frac{3t}{4T} \times \frac{bbxx}{2p}$.

When the sun arrives at the solstitial point C, then x becomes equal to 1, and the nutation is then $360 \times \frac{3t}{4T} \times \frac{bb}{2p} = 180 \times \frac{3t}{4T} \times \frac{a^2-e^2}{a^2} \times \frac{b}{p}$ in degrees, or $10800 \times \frac{3t}{4T} \times \frac{a^2-e^2}{a^2} \times \frac{b \times 60}{p}$ in seconds. Now $t =$ one sidereal day, $T = 366\frac{1}{4} = \frac{1465}{4}$, and therefore $4T = 1465$. According to Sir ISAAC NEWTON's determination of the figure of the earth, a is as 231, e as 230, and therefore $\frac{a^2-e^2}{a^2} = \frac{461}{53361}$. Also supposing the obliquity of the ecliptic 23° 27′ 45″, $b = .3981487$, and $p = 3.14159265$. Consequently $10800 \times \frac{3t}{4T} \times \frac{a^2-e^2}{a^2} \times \frac{b \times 60}{p} = 10800 \times \frac{3}{1465} \times \frac{461}{53361} \times \frac{23.888922}{3.14159265}$, the computation of which may be finished in the following manner.

10800	Log.	4.0334238	1465	Log. - -	3.1658376
3	Log.	0.4771213	53361	Log. - -	4.7272240
461	Log.	2.6637009	3.14159265	Log.	0.4971499
23.888922	Log.	1.3781998			8.3902115
		8.5524458			
		8.3902115			
		0.1622343			

= Log. of 1″.4529, and there-

fore the nutation caused by the action of the sun in a quarter of a year is $1'' \; 27'''$ nearly.

25. By the circular parts, radius $\times c =$ cot. LS $\times$ tang. BL $= c$, radius being 1. But $x : \sqrt{1-xx} :: 1 : \frac{\sqrt{1-xx}}{x} =$ cot. LS; and therefore $\frac{cx}{\sqrt{1-xx}} =$ tang. BL. Consequently by the last and article 23, $b : \frac{cx}{\sqrt{1-xx}} :: 360 \times \frac{3t}{2T} \times \frac{bbx\dot{x}}{2p} : 360 \times \frac{3ct}{4T} \times \frac{b}{p} \times \frac{x^2\dot{x}}{\sqrt{1-xx}} =$ the fluxion of the precession when the sun is at S. Now the fluent of $\frac{x^2\dot{x}}{\sqrt{1-xx}}$ is $\frac{z-x\sqrt{1-xx}}{2}$. For $\dot{z} = \frac{\dot{x}}{\sqrt{1-xx}}$, and the fluxion of $x\sqrt{1-xx}$ is $\dot{x}\sqrt{1-xx} - \frac{x^2\dot{x}}{\sqrt{1-xx}}$. Consequently the whole fluxion of $z - x\sqrt{1-xx}$ is $\frac{\dot{x}}{\sqrt{1-xx}} - \dot{x}\sqrt{1-xx} + \frac{x^2\dot{x}}{\sqrt{1-xx}} = \frac{\dot{x}}{\sqrt{1-xx}} - \frac{\dot{x}}{\sqrt{1-xx}} + \frac{x^2\dot{x}}{\sqrt{1-xx}} + \frac{x^2\dot{x}}{\sqrt{1-xx}} = \frac{2x^2\dot{x}}{\sqrt{1-xx}}$. Consequently the fluent of the precession, when the sun is at S, is $360 \times \frac{3ct}{4T} \times \frac{b}{p} \times \frac{z-x\sqrt{1-xx}}{2}$.

When the sun arrives at the solstitial point C, then x becomes equal to 1, and z becomes equal to $\frac{p}{2}$, and the quantity of the precession is then $360 \times \frac{3ct}{4T} \times \frac{b}{p} \times \frac{p}{4} = 360 \times \frac{3t}{4T} \times \frac{cb}{4}$. This expressed in numbers, admitting the obliquity of the ecliptic to be $23^\circ \; 27' \; 45''$, is $90 \times \frac{3}{1465} \times \frac{461}{53361} \times .9173813$ in degrees, and the same in seconds is $5400 \times \frac{3}{1465} \times \frac{461}{53361} \times 55.042878$. This calculation may be finished in the following manner.

5400	Log. 3.7323938	1465 -	Log. 3.1658376
3	Log. 0.4771213	53361 -	Log. 4.7272240
461	Log. 2.6637009		7.8930616
55.042878	Log. 1.7407010		
	8.6139170		
	7.8930616		
	0.7208554 = Log. of 5″.2584.		

Consequently the annual precession, caused by the disturbing force of the sun, is 21″.0336.

The obliquity of the ecliptic has been assumed as equal to 23° 27′ 45″, such being its magnitude, very nearly, at the beginning of the year 1807.

From the general expression $360 \times \frac{3t}{4T} \times \frac{hbx.x}{2p}$, obtained in article 24, it is evident, that when the sun is in either of the equinoctial points, the nutation becomes equal to 0. Supposing therefore the earth to be subject to no other disturbing force than that of the sun, at each of the equinoxes the earth's diurnal revolution is made about its axis of figure, as PL in Fig. 9; but as at other times the disturbing force tends to cause a libration about a diameter of the equator, it is evident from article 10, that the axis about which it revolves deviates, by a quantity extremely small, from its axis of figure. A similar deviation, of the axis of revolution from the axis of figure, is produced by the action of the moon; but a minute examination of these deviations is foreign to the present design. As the foregoing articles extend beyond the supposed difficulty in the subject, it is deemed unnecessary either to add to their number, or to lengthen this Paper by such additional remarks, as may be met with in every respectable publication on Physical Astronomy.

Fig. 2.

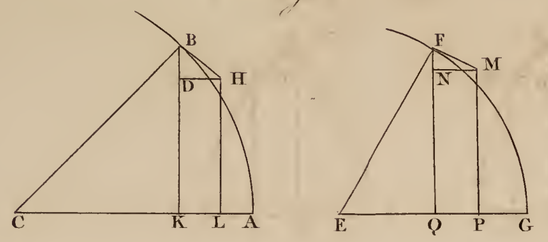


Fig. 7.

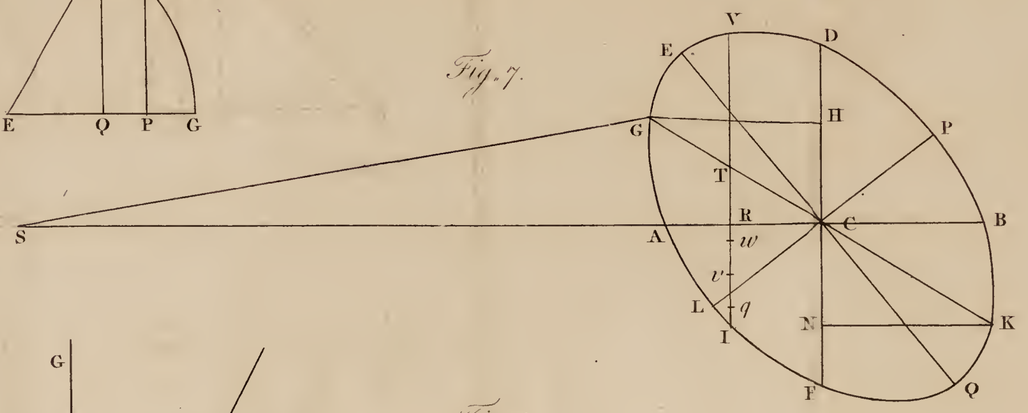


Fig. 5.

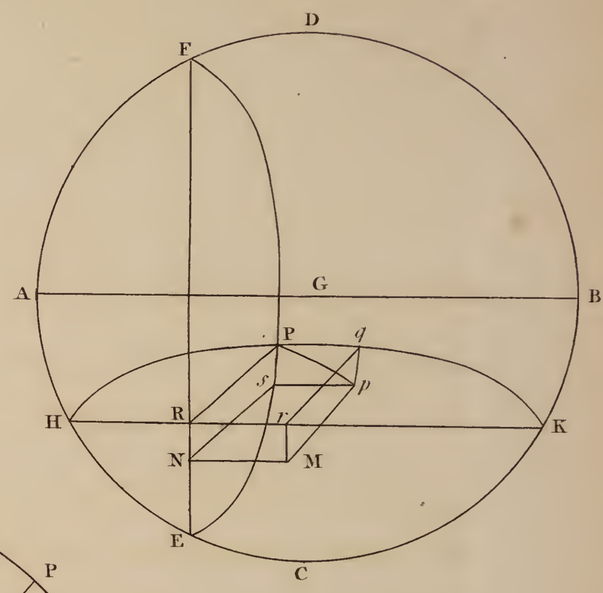
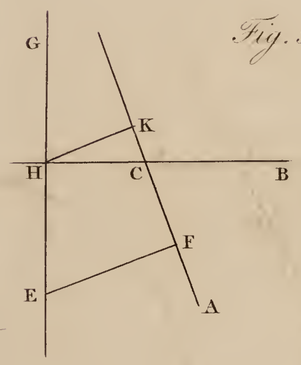


Fig. 3.



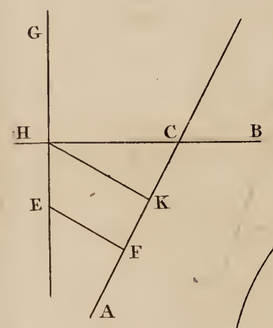


Fig. 6.

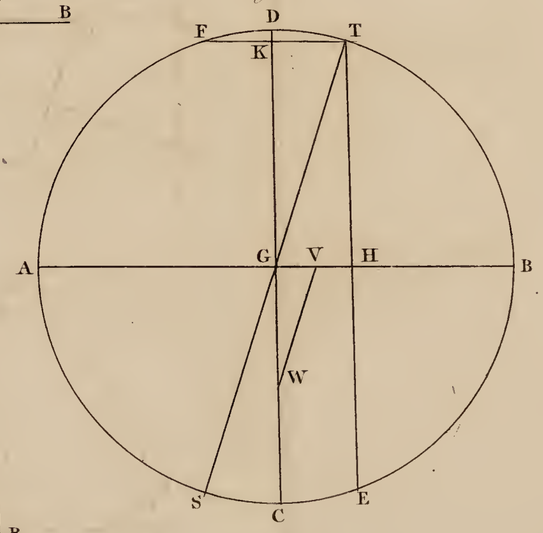


Fig. 8.

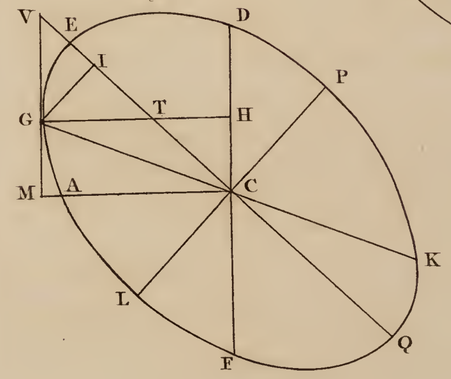


Fig. 10.

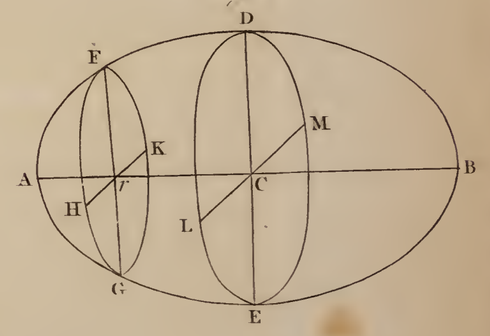


Fig. 4.

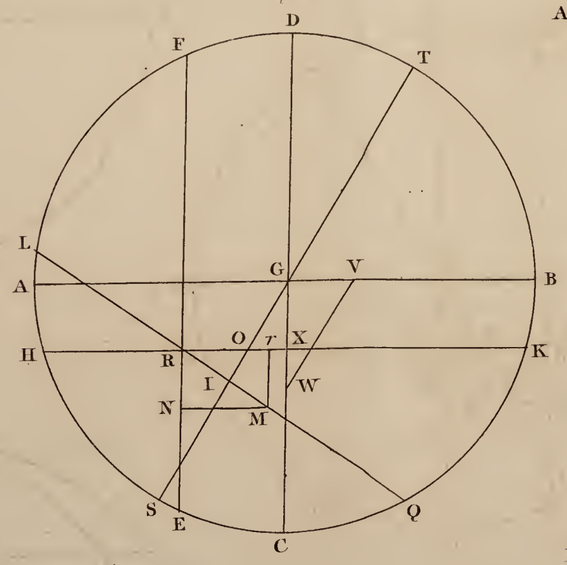


Fig. 11.

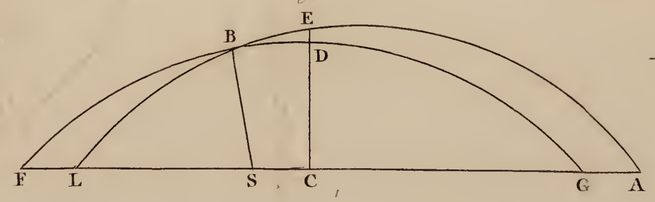


Fig. 9.

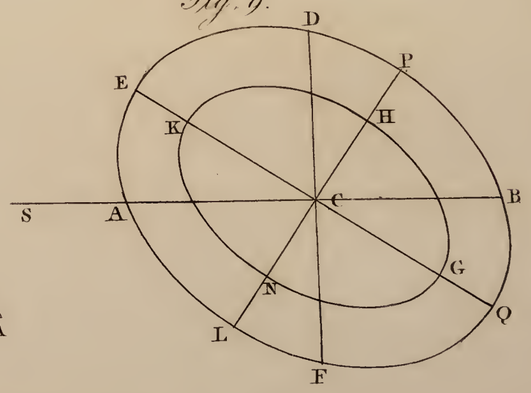
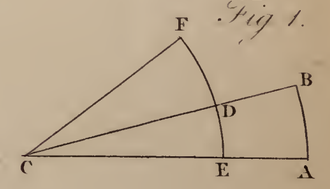


Fig. 1.



Basire sc.

III. *An Account of two Children born with Cataracts in their Eyes, to shew that their Sight was obscured in very different Degrees; with Experiments to determine the proportional Knowledge of Objects acquired by them immediately after the Cataracts were removed.* *By* Everard Home, *Esq. F. R. S.*

Read January 15, 1807.

Mr. Cheselden's observations on this subject, recorded in the Phil. Trans. for the year 1728, pointed out two material facts; that vision alone gives no idea of the figure of objects, or their distance from the eye, since a very intelligent boy, 13 years of age, upon recovering his sight was unable to distinguish the outline of any thing placed before him, and thought that every object touched his eye.

Mr. Ware's cases, which have also a place in the Phil. Trans. for 1801, and are compared with that of Mr. Cheselden, appear to lead to a different conclusion. The following observations are laid before the Society with a view to explain this circumstance.

CASE I.

William Stiff, twelve years of age, was admitted into St. George's Hospital under my care, on the 17th of July, 1806, with cataracts in his eyes, which, according to the account of his mother, existed at the time of birth. From earliest

infancy he never stretched out his hand to catch at any thing, nor were his eyes directed to objects placed before him, but rolled about in a very unusual manner, although in other respects he was a lively child. The eyes were not examined till he was six months old, and at that time the cataracts were as distinct as when he was received into the hospital.

Previous to an operation being performed, the following circumstances were ascertained respecting his vision. He could distinguish light from darkness, and the light of the sun from that of a fire or candle: he said it was redder, and more pleasant to look at, but lightning made a still stronger impression on his eyes. All these different lights he called red. The sun appeared to him the size of his hat. The candle flame was larger than his finger, and smaller than his arm. When he looked at the sun he said it appeared to touch his eye. When a lighted candle was placed before him both his eyes were directed towards it, and moved together. When it was at any nearer distance than 12 inches, he said it touched his eyes. When moved further off he said it did not touch them; and at 22 inches it became invisible.

On the 21st of July the operation of extracting the crystalline lens was performed on the left eye. The capsule of the lens was so very strong as to require some force to penetrate it. When wounded, the contents, which were fluid, rushed out with great violence. Light became very distressing to his eye, and gave him pain. After allowing the eye-lids to remain closed for a few minutes, and then opening them, the pupil appeared clear, but he could not bear exposure to light. On my asking him what he had seen, he said, "your head, which

seemed to touch my eye:" but he could not tell its shape. He went to bed, and took an opiate draught: the pain in his eye lasted about an hour, after which he fell asleep. The whole of that day the light was distressing to his eye, so that he could not bear the least exposure to it.

On the 22d the eye-lids were opened to examine the eye. The light was less offensive. He said he saw my head, which touched his eye. There was so much inflammation on the eye-ball, that a leech was applied to the temple, and the common means for removing inflammation were used.

On the 23d the eye was less inflamed, and he could bear a weak light. The pupil was of an irregular figure, and the wounded cornea had not united with a smooth surface. He said he could see several gentlemen round him, but could not describe their figure. My face, while I was looking at his eye, he said was round and red.

On the 25th the inflammation had subsided, but on the 27th returned, and continued notwithstanding different means were employed for its removal, till the 1st of August, when it was almost entirely gone. On the 4th the eye was apparently so well that an attempt was made in the presence of Mr. CAVENDISH and Dr. WOLLASTON to ascertain its powers of vision; but it was so weak that it became necessary to shade the glare of light by hanging a white cloth before the window. The least exertion fatigued the eye, and the cicatrix on the cornea, to which the iris had become attached, drew it down so as considerably to diminish the pupil. From these circumstances nothing could be satisfactorily made out respecting the boy's vision. On the 11th a second attempt was made in the presence of Mr. CAVENDISH, but the pupil conti-

nued so contracted and irregular, and the eye so imperfect in its powers, that it became necessary a second time to postpone any experiments.

On the 16th of September the right eye was couched. This operation was preferred after what had happened to the other eye, in the hope that there would not be the same degree of inflammation, and as the former cataract was fluid, there was every reason to believe that couching would in this instance be most efficacious.

The operation gave pain, and the light was so distressing to his eye that the lids were closed as soon as it was over, and he was put to bed. The consequent inflammation was not severe, but as soon as the fluid cataract, which had been diffused through the aqueous humour was absorbed, the capsule of the lens was found to be opaque, and the sight consequently imperfect. The eyes were not examined with respect to their vision till the 13th of October, during which period the boy remained quiet in the hospital. On that day the upper part of the pupil of the left eye had in some measure recovered its natural state, and had become transparent, but the cicatrix in the cornea was more extensively opaque than before. The light now was not distressing to either eye, and when strong, he could readily discern a white, red, or yellow colour, particularly when bright and shining. The sun and other objects did not now seem to touch his eyes as before, they appeared to be at a short distance from him. The eye, which had been couched, had the most distinct vision of the two, but in both it was imperfect. The distance at which he saw best was five inches.

When the object was of a bright colour, and illuminated

by a strong light, he could make out that it was flat and broad; and when one corner of a square substance was pointed out to him, he saw it, and could find out the other, which was at the end of the same side, but could not do this under less favourable circumstances. When the four corners of a white card were pointed out, and he had examined them, he seemed to know them: but when the opposite surface of the same card, which was yellow, was placed before him, he could not tell whether it had corners or not, so that he had not acquired any correct knowledge of them, since he could not apply it to the next coloured surface, whose form was exactly the same, with that, the outline of which the eye had just been taught to trace.

CASE II.

John Salter, seven years of age, was admitted into St. George's Hospital on the 1st of October, 1806, under my care, with cataracts in both eyes, which according to the accounts of his relations had existed from his birth.

After he was received into the hospital, the following circumstances were ascertained respecting his vision. The pupils contracted considerably when a lighted candle was placed before him, and dilated as soon as it was withdrawn. He was capable of distinguishing colours with tolerable accuracy, particularly the more bright and vivid ones.

On the 6th of October the left eye was couched. This operation was preferred to extraction, from a belief that the cataracts were not solid, and as the injury done to the capsule by the operation would be less, there was not the same chance of inflammation, the disposition for which, had been so

strong in the former case. As the eye was not irritable, and was likely to be but little disturbed by this operation, every thing was previously got ready for ascertaining his knowledge of objects, as soon as the operation was over, should the circumstances prove favourable. The operation was attended with success, and gave very little pain. The eye was allowed ten minutes to recover itself: a round piece of card of a yellow colour, one inch in diameter, was then placed about six inches from it. He said immediately that it was yellow, and on being asked its shape, said, " Let me touch " it, and I will tell you." Being told that he must not touch it, after looking for some time, he said it was round. A square blue card, nearly the same size, being put before him, he said it was blue and round. A triangular piece he also called round. The different colours of the objects placed before him he instantly decided on with great correctness, but had no idea of their form. He moved his eye to different distances, and seemed to see best at 6 or 7 inches. His focal distance has been since ascertained to be 7 inches. He was asked whether the object seemed to touch his eye, he said " No;" but when desired to say at what distance it was, he could not tell. These experiments were made in the theatre of the hospital, in which the operation was performed, before the surgeons and all the students. He was highly delighted with the pleasure of seeing, and said it was " so pretty," even when no object was before him, only the light upon his eye. The eye was covered, and he was put to bed, and told to keep himself quiet, but upon the house-surgeon going to him half an hour afterwards, his eye was found uncovered, and he was looking at his bed curtains, which were close drawn.

The bandage was replaced, but so delighted was the boy with seeing, that he again immediately removed it. This circumstance distressed the house-surgeon, who had been directed to prevent him from looking at any thing till the next day, when the experiment was to be repeated. Finding that he could not enforce his instructions, he thought it most adviseable to repeat the experiment about two hours after the operation. At first the boy called the different cards round; but upon being shewn a square, and asked if he could find any corners to it, he was very desirous of touching it. This being refused, he examined it for some time, and said at last that he had found a corner, and then readily counted the four corners of the square; and afterwards when a triangle was shewn him, he counted the corners in the same way; but in doing so his eye went along the edge from corner to corner, naming them as he went along.

Next day, when I saw him, he told me he had seen "the "soldiers with their fifes and pretty things." The guards in the morning had marched past the hospital with their band; on hearing the music he had got out of bed, and gone to the window to look at them. Seeing the bright barrels of the musquets, he must in his mind have connected them with the sounds which he heard, and mistaken them for musical instruments. On examining the eye 24 hours after the operation, the pupil was found to be clear. A pair of scissors was shewn him, and he said it was a knife. On being told he was wrong, he could not make them out; but the moment he touched them he said they were scissors, and seemed delighted with the discovery. On being shewn a guinea at the distance of 15 inches from his eye, he said it was a seven

shilling piece, but placing it about 5 inches from his eye, he knew it to be a guinea; and made the same mistake, as often as the experiment was repeated.

From this time he was constantly improving himself by looking at, and examining with his hands, every thing within his reach, but he frequently forgot what he had learnt. On the 10th I saw him again, and I told him his eye was so well that he might go about as he pleased without leaving the room. He immediately went to the window, and called out, "What is that moving?" I asked him what he thought it was? He said, "A dog drawing a wheelbarrow. There is one, two, three dogs drawing another. How very pretty!" These proved to be carts and horses on the road, which he saw from a two pair of stairs window.

On the 19th, the different coloured pieces of card were separately placed before his eye, and so little had he gained in thirteen days, that he could not without counting their corners one by one tell their shape. This he did with great facility, running his eye quickly along the outline, so that it was evident he was still learning, just as a child learns to read. He had got so far as to know the angles, when they were placed before him, and to count the number belonging to any one object.

The reason of his making so slow a progress was, that these figures had never been subjected to examination by touch, and were unlike any thing he was accustomed to see.

He had got so much the habit of assisting his eyes with his hands, that nothing but holding them could keep them from the object.

On the 26th the experiments were again repeated on the

couched eye, to ascertain the degree of improvement which had been made. It was now found that the boy, on looking at any one of the cards in a good light, could tell the form nearly as readily as the colour,

From these two cases the following conclusions may be drawn:

That, where the eye before the cataract is removed, has only been capable of discerning light, without being able to distinguish colours, objects after its removal will seem to touch the eye, and there will be no knowledge of their outline; which confirms the observations made by Mr. CHESELDEN:

That where the eye has previously distinguished colours, there must also be an imperfect knowledge of distances, but not of outline, which however will afterwards be very soon acquired, as happened in Mr. WARE's cases. This is proved by the history of the first boy in the present Paper, who before the operation had no knowledge of colours or distances, but after it, when his eye had only arrived at the same state, that the second boy's was in before the operation, he had learnt that the objects were at a distance, and of different colours: that when a child has acquired a new sense, nothing but great pain or absolute coercion, will prevent him from making use of it.

In a practical view, these cases confirm every thing, that has been stated by Mr. POTT and Mr. WARE, in proof of cataracts in children being generally soft, and in favour of couching, as being the operation best adapted for removing them. They also lead us to a conclusion of no small importance, which has not before been adverted to; that when the cataract has assumed a fluid form, the capsule, which is

naturally a thin transparent membrane, has to resist the pressure of this fluid, which like every other diseased accumulation is liable to increase, and distend it, and therefore the capsule is rendered thicker and more opaque in its substance, like the coats of encysted tumours in general.

As such a change is liable to take place, the earlier the operation is performed in all children, who have cataracts completely formed, the greater is their chance of having distinct vision after the operation. It is unnecessary to point out the advantages to be derived from its being done at a more early age, independent of those respecting the operation itself.

IV. *Observations on the Structure of the different Cavities, which constitute the Stomach of the Whale, compared with those of ruminating Animals, with a View to ascertain the Situation of the digestive Organ.* *By* Everard Home, *Esq. F. R. S.*

Read February 12, 1807.

THE following observations are in some measure a continuation of those upon the stomachs of ruminating animals contained in a former Paper. They are intended to show that the stomach of the whale forms a link in the gradation towards the stomachs of truly carnivorous animals.

This subject was brought under my consideration by the following circumstances. While at Worthing, on the Sussex coast, in the month of August last, a Delphinus Delphis of LINNÆUS, or small bottle-nose whale of Mr. HUNTER, was brought on shore by the fishermen alive. I immediately purchased it, with a view of enriching the HUNTERIAN collection with the skeleton, and other parts of its structure.

The stomach was the particular object of my own attention; for, having been so lately employed in considering the stomachs of ruminating animals, I was pleased with an opportunity of examining in a recent state the stomach of one of the whale tribe, to which the porpoise belongs, with a view to ascertain more accurately than had been hitherto done, the real resemblance between its structure, and that of the stomachs of ruminating animals.

The structure of the stomach of one species of whale was not new to me, having twenty years ago assisted Mr. HUNTER in dissecting the piked whale, but at that time I only viewed the different parts of its structure with the eye of a common observer, while now my mind was particularly directed to the peculiarities of the stomach. In this examination I discovered a resemblance between the second, third, and fourth cavities in the whale, and the two portions of the fourth cavity in the bullock and camel, which appears to throw some light upon the uses of those parts, as well as upon digestion in general.

As in the former Paper a particular description was given of the stomach of the bullock and camel, as examples of ruminants with and without horns, it will be proper here to describe the stomach of the bottle-nose porpoise, as an example of the whale tribe.

In the bottle-nose porpoise the œsophagus is very wide, has a number of longitudinal folds, and is lined with a strong white cuticle, which is continued over the internal surface of the first stomach.

The first stomach lies in the direction of the œsophagus, which is continued into it, there being no contraction to mark its origin. It is of an oval form, and bears a strong resemblance in shape to a Florence flask. The cavity is 15 inches in length, and 9 in diameter. The internal surface has a very corrugated appearance, and its cuticular covering is thick and strong. The coats of the cavity are firm, and its bottom is surrounded by a strong muscular covering.

The orifice, which leads to the second stomach, is at right angles to the cavity, and is situated a little way below the

termination of the œsophagus. It is surrounded by several semicircular doublings of the internal membrane: the broadest of these is on the lower part, these are thick, and appear to be glandular.

There is a canal between the first and second cavities 3 inches long, which opens into the second by a projecting orifice, and the cuticular covering of the first stomach terminates immediately beyond this orifice, which is $2\frac{1}{2}$ inches in diameter.

This second stomach is nearly spherical, about 7 inches in diameter. Its internal surface has a honeycombed appearance, formed by soft ridges of a glandular structure, leaving interstices of some depth between them. This structure gives the coats a considerable degree of thickness.

The opening into the third stomach is almost close to that which enters the second, and is only $\frac{5}{8}$ of an inch in diameter.

The third cavity is nearly spherical, and is 2 inches in diameter. Its internal surface is smooth, and there are every where small orifices of ducts of glands opening into its cavity. The aperture, which communicates between this and the fourth stomach is $\frac{3}{8}$ of an inch in diameter.

The fourth cavity is nearly cylindrical like an intestine, but rather widest at its furthest extremity. It is $14\frac{1}{2}$ inches long; its greatest diameter is 3 inches. The internal membrane is smooth, and for 3 inches towards its origin, and 4 inches towards its termination has numerous orifices through which secretions are poured into the cavity. The pylorus, which is the boundary of this stomach, is a round orifice $\frac{2}{8}$ of an inch in diameter.

Immediately beyond the pylorus there is a dilatation of the gut, which both CUVIER and HUNTER call a cavity belonging to the stomach. It must however be considered as duodenum, since the common duct of the liver and pancreas opens into it; the longitudinal *valvulæ conniventes* have their origin in it; and there is no transverse constriction any where beyond it, to mark the beginning of an intestine. Such an enlargement of the duodenum is very common in other animals, and has been described in the account of the camel. The coats of this portion of the duodenum are thicker than those of the fourth stomach. The annexed drawings (Plate III.) will give a better idea of these different parts than can be conveyed by any verbal description.

The number of cavities constituting the stomach are not the same in all animals of the whale tribe. In the common porpoise, grampus, and piked whale, the number is the same as in the bottle-nose porpoise; but in the bottle-nose whale of Dale there are two more cavities. This variation is however by no means material, since the general structure of the stomach is the same.

In all of the whale tribe there is one cavity lined with a cuticle, as in the bullock and camel.

In all of them there is a second cavity made up of a very glandular structure. In the porpoise, grampus, and large bottle-nose whale this structure resembles that which is above described. In the piked whale the rugæ are longitudinal and deep, but in some places united by cross bands; and as the piked whale has whalebone teeth, the great whalebone whale will probably, from the analogy of its teeth, resemble it in the structure of its stomach.

The third cavity in all of them is very small, and bears a strong resemblance to the third cavity in the camel's stomach; its use, therefore, is probably the same.

The fourth stomach in all of them has a smooth internal surface, with the orifices of glands opening into its cavity. In the bottle-nose whale of Dale the two additional cavities have the same internal structure, and therefore must have the same general use, with a greater extension of surface, and the subdivisions will make the food pass more slowly into the intestine.

The first stomach of the whale is not only a reservoir, but the food undergoes a considerable change in it. The flesh is entirely separated from the bones in this cavity, which proves that the secretion from the glandular part has a solvent power. This was found to be the case in the bottle-nose porpoise and large bottle-nose whale. In both of them several handfuls of bones were found in the first stomach, without the smallest remains of the fish, to which they belonged. The soft parts only can be conveyed into the second and third stomachs, the orifices being too small to admit the bones to pass.

The bones must therefore be reduced to a jelly in the first stomach, and although the process, by which this is effected, being slower than that, which separates the flesh, is the reason of their being found in such quantity in the cavity, the means by which it is performed are probably the same.

The second cavity was supposed by Mr. Hunter to be the true digesting stomach, in which the food becomes chyle, and the use of the third and fourth he looked upon as not exactly ascertained.*

* *Vide* Observations on the Structure and Œconomy of Whales. By John Hunter. Phil. Thrans. Vol. LXXVII. page 411.

Upon what ground Mr. HUNTER was led to draw this conclusion cannot now be ascertained; and, such is my respect for his opinion, that nothing but the following observations, supported by facts, could lead me to form a different one. In considering this subject, it struck me that the second stomach, could not be that, in which chyle is formed, since that process having been completed, any other cavities would be superfluous. The last cavity in all stomachs is that, in which the process must be brought to perfection: and therefore the most essential change, which the food undergoes, or that, by which it is formed into chyle should be performed in that cavity. Surveying the different cavities, in the whale's and ruminating stomachs with this impression on my mind, and comparing them with the single stomachs of carnivorous animals, it appeared that the first point, which required to be ascertained was, which of the cavities in these more complex stomachs bears the greatest resemblance to the simple one. The fourth of the whale is certainly more like the human stomach than the second or third. I therefore concluded that the fourth, both from analogy and situation, is the stomach in which the process is completed: and that in this animal, from the peculiarities of its œconomy, and the nature of the food, not only a cuticular stomach is necessary, but also two glandular ones, in which it undergoes changes preparatory to its being converted into chyle.

Having satisfied myself upon this subject, and having compared the stomachs of the whale, with the fourth of the camel, the contraction or partial division of the camel's, made it apparent that the lower portion only of that cavity, which resembles in shape, and internal appearance the human

stomach, is the cavity in which chyle is formed, and the upper or plicated portion is only to prepare the food, and is therefore analogous to the second in the whale.

As the same appearances are met with in the fourth stomach of the bullock, as well as in the camel, although there is no permanent contraction, or division between them, the upper or plicated portion must be considered as a preparatory organ, and the lower portion as that, in which the formation of chyle is compleated. This receives further confirmation from a more attentive examination of the parts, immediately after death, by which it was found that before the stomach has been disturbed there is an evident muscular contraction between the plicated and lower portion. This appearance was met with in every instance that was examined, and these were not fewer than nine or ten. Added to this the lower portion, on a more minute inspection, has an appearance somewhat similar to the inner membrane of the human stomach: and the surface of the plicæ is in many respects different.

From the facts and observations which have been stated, it appears that in many animals of the class Mammalia, the food undergoes different changes preparatory to its being converted into chyle, and this last process is effected by a somewhat similar secretion, since the part of the stomach which produces it, has in all of them an evident similarity of structure.

The above facts appear to throw some light on the digestion of the different kinds of food, and open a wide field of enquiry into one of the most interesting parts of the animal œconomy, which has been hitherto too much neglected. In the present very limited state of our knowledge there are

many circumstances which cannot be accounted for: these however, will be explained when a further progress has been made in this investigation.

It is obvious, that as the stomachs of carnivorous animals are the most simple, animal substances, on which they feed, require a shorter process to convert them into chyle than vegetables; but why the whale tribe, which live on fish, should have a more complex stomach, it is not easy to explain: since fish are very readily converted into chyle, in the stomachs of animals of their own class, as well as in the human stomach, and there is therefore reason to believe that they require as little preparation for that process, if not less than animal substances.

The fish bones swallowed by the whale tribe being retained in the cuticular bag, till they are reduced to jelly, explains the circumstance of cows, and other ruminating animals being able occasionally to live on fish, (a fact, of which there is no doubt, both in the Orkneys and in Iceland,) since, if the bones are dissolved in the paunch, the other stomachs, are in no danger of being injured from the animal living on this kind of food.

Whether these cavities, which I have called preparatory stomachs, are solely for purposes connected with digestion, or are also in any way connected with the formation of secretions peculiar to those animals, cannot be ascertained in the present state of our knowledge of digestion.

The oil of the physeter, which crystallizes into spermaceti, shews some affinity in this respect to the secretion of fat, that becomes suet, which is only met with in ruminating animals: but on the other hand, the oil of the rest of the

whale tribe does not form this substance, more than the fat of the horse produces tallow. These facts may be afterwards explained by an examination of the digestive organs of the physeter, when an anatomist shall have an opportunity of examining them.

These are enquiries which do not belong to the present Paper, as it is only intended to add some facts to those already laid before the Society, and in a future communication I hope still further to increase their number.

EXPLANATION OF THE PLATES.

(PLATE III.)

This plate represents the first cavity of the stomach of the bottle-nose porpoise laid open to shew its internal structure.

a a. The œsophagus lined with cuticle.

b b. The first cavity of the stomach, also lined with cuticle.

c c. The glandular structure forming folds round the orifice leading to the second cavity, also lined with cuticle.

(PLATE IV.)

This plate represents the internal surface of the second, third, and fourth cavities of the stomach.

a a. The outside of the first cavity to shew its external form.

b b. The inner surface of the second cavity, made up of a honey-combed structure, composed of soft membraneous folds, which have no cuticular covering.

c c. The third cavity.

d d. The fourth cavity.

e e. The orifices of excretory ducts of glands.

f f. The enlargement of the substance immediately beyond the pylorus, into which the common duct from the liver and pancreas opens.

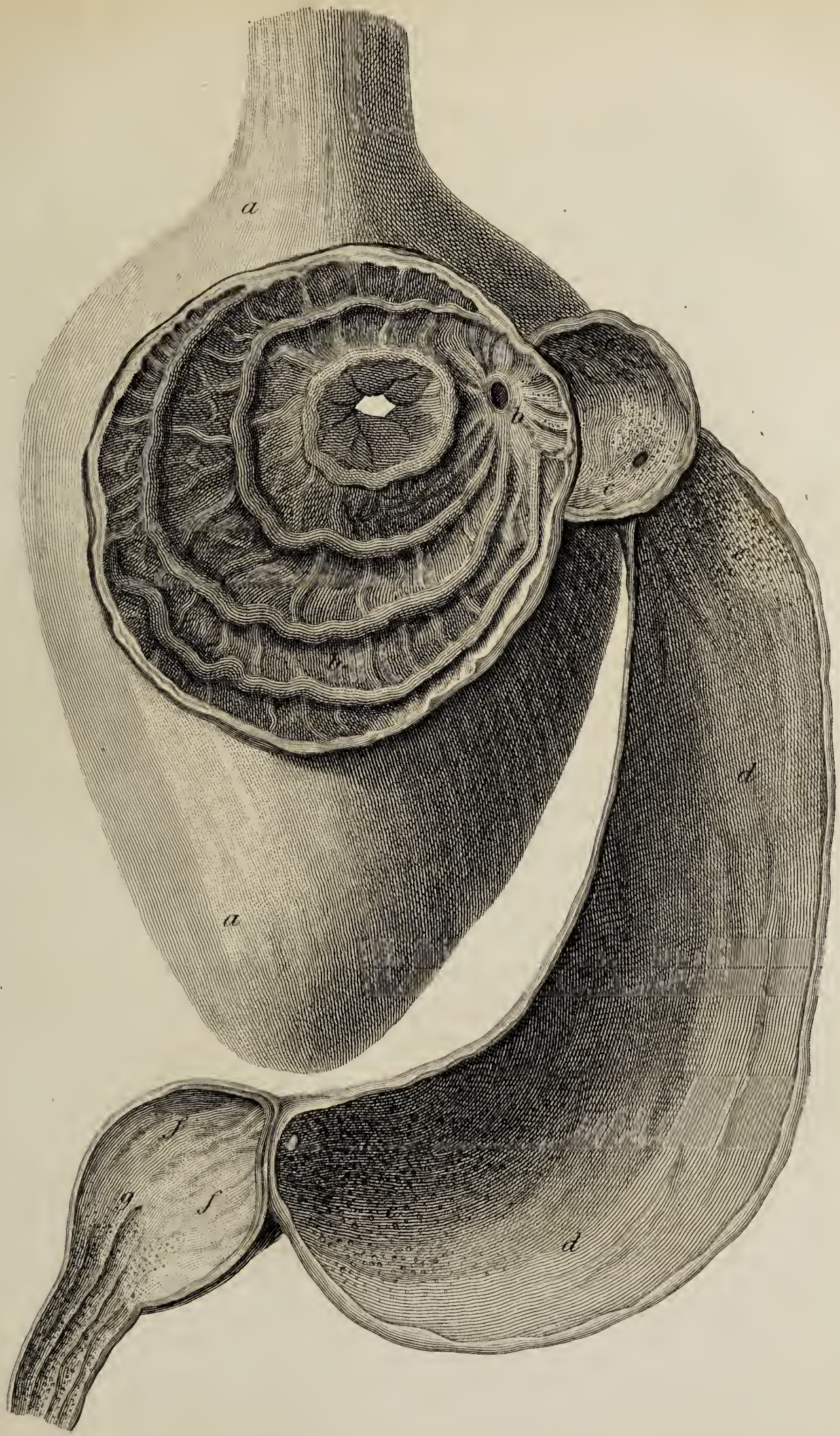
g. The opening of the common duct.



SCALE—*Six Inches to a Foot.*

Wm Clift del.

Basire sc.



*SCALE*_ *Five Inches to a Foot*.

IV. *On the Formation of the Bark of Trees. In a Letter from* T. A. Knight, *Esq. F. R. S. to the Right Honourable Sir* Joseph Banks, K. B. P. R. S. *&c.*

Read February 19, 1807.

My Dear Sir,

An extraordinary diversity of opinion appears to have prevailed among naturalists, respecting the production and subsequent state of the bark of trees.

According to the theory of Malpighi, the cortical substance, which is annually generated, derives its origin from the older bark; and the interior part of this new substance is annually transmuted into alburnum, or sap wood; whilst the exterior part, becoming dry and lifeless, forms the exterior covering, or cortex.

The opinions of Grew do not appear to differ much from those of Malpighi; but he conceives the interior bark to consist of two distinct substances, one of which becomes alburnum, whilst the other remains in the state of bark: he, however, supposes the insertments in the wood, the "utriculi" of Malpighi, and the "tissu cellulaire" of Du Hamel, to have originally existed in the bark.

Hales on the contrary contends, that the bark derives its existence from the alburnum, and that it does not undergo any subsequent transformation.

The discoveries of Du Hamel have thrown much light

on the subject; but his experiments do not afford any conclusive result, and some of them may be adduced in support of either of the preceding hypotheses: and a modern writer (MIRBEL*) has endeavoured to combine and reconcile, in some degree, the apparently discordant theories of MALPIGHI and HALES. He contends with HALES, that the alburnum gives existence to the new layer of bark; but that this bark subsequently changes into alburnum, though not precisely in the manner described by MALPIGHI.

So much difference of opinion, amongst men so capable of observing, sufficiently evinces the difficulty of the subject they endeavoured to investigate: and in a course of experiments, which has occupied more than twenty years, I have scarcely felt myself prepared, till the present time, even to give an opinion respecting the manner, in which the cortical substance is generated in the ordinary course of its growth; or reproduced, when that, which previously existed, has been taken off.

DU HAMEL has shewn, that the bark of some species of trees is readily reproduced, when the decorticated surface of the alburnum is secluded from the air; and I have repeated similar experiments on the apple, the sycamore, and other trees, with the same result; I have also often observed a similar reproduction of bark on the surface of the alburnum of the *Wych elm* (Ulmus montana) *in shady situations*, when no covering whatever was applied. A glareous fluid, as DU HAMEL has stated, exudes from the surface of the alburnum: this fluid appears to change into a pulpous unorganised mass, which subsequently becomes organised and cellular; and the

* Traité d'Anatomie et de Physiologie vegetales

matter, which enters into the composition of this cellular substance, is evidently derived from the alburnum.

These facts are therefore extremely favourable to the theory of HALES; but other facts may be adduced which are scarcely consistent with that theory.

The internal surface of pieces of bark, when detached from contact with the alburnum, provided they remain united to the tree at their upper ends, much more readily generate a new bark, than the alburnum does under similar circumstances: a similar fluid exudes from the surfaces of both, and the same phenomena are observable in both cases. The cellular substance, however, which is thus generated, though it presents every external appearance of a perfect bark, is internally very imperfectly organized; and the vessels which contain the true sap in the bark, are still wanting; and I have found, that these may be made, by appropriate management, to traverse the new cellular substance in almost any direction. When I cut off all communication above, and on one side, between the old bark and that substance, I observed, that the vessels proceeded across it, from the old bark on the other side, taking always in a greater or less degree an inclination downwards; and when the cellular substance remained united to the bark at its upper end only, the vessels descended nearly perpendicularly down it; but they did not readily *ascend* into it, *when it was connected with the bark at its lower extremity only;* the result of similar experiments, when made on different species of trees, was, however, subject to some variations.

Pieces of bark of the walnut-tree, which were two inches

broad, and four long, having been detached from contact with the alburnum, except at their upper ends, and covered with a plaister composed of bees-wax and turpentine, in some instances, and with clay only in others, readily generated the cellular substance of a new bark; and between that and the old detached bark, very nearly as much alburnum was deposited as in other parts of the tree, where the bark retained its natural position; which, I think, affords very decisive evidence of the descent of the sap through the bark. Similar pieces of bark, under the same mode of treatment, but united to the tree at their lower ends only, did not long remain alive, except at their lower extremities; and there a very little alburnum only was generated. Other pieces of bark of the same dimensions, which were laterally united to the tree, continued alive almost to their extremities; and a considerable portion of alburnum was generated, particularly near their lower edges; the sap appearing in its passage across the bark to have been given a considerable inclination downwards: probably owing to an arrangement in the organization of the bark, that I have noticed in a former memoir,* which renders it better calculated to transmit the sap towards the roots than in any other direction.†

I have in very few instances been able to make the walnut-tree reproduce its bark from the alburnum, though under the same management I rarely failed to succeed with the sycamore and apple-tree. Pieces of the bark of the apple-tree will also live, and generate a small portion of alburnum, though only attached to the tree at their lower extremities;

* Philosophical Transactions of 1804.

probably owing to a small part of the true sap being carried upwards by capillary attraction, when the proper action of the cortical vessels is necessarily suspended.

The preceding experiments, and the authority of Du Hamel, having perfectly satisfied me, that both the alburnum and bark of trees are capable of generating a new bark, or at least of transmitting a fluid capable of generating a cellular substance, to which the bark in its more perfectly organized state owes its existence, my attention was directed to discover the sources from which this fluid is derived. Both the bark and the alburnum of trees are composed principally of two substances; one of which consists of long tubes, and the other is cellular; and the cellular substance of the bark is in contact with the similar substance in the alburnum, and through these I have long suspected the true sap to pass from the vessels of the bark to those of the alburnum.* The intricate mixture of the cellular and vascular substances long baffled my endeavours to discover from which of them, in the preceding cases, the sap, and consequently the new bark, proceeded; but I was ultimately successful.

The cellular substance, both in the alburnum and bark of old pollard oaks, often exists in masses of near a line in width, and this organization was peculiarly favourable to my purpose. I therefore repeated on the trunks of trees of this kind, experiments similar to those above-mentioned which were made on the walnut-tree.

Apparently owing to the small quantity of sap, which the old pollard trees contained, their bark was very imperfectly reproduced; but I observed a fluid to ouze from the cellular

* Phil. Trans. 1805, page 14.

substance, both of the bark and alburnum; and on the surface of these substances alone, in many instances, the new bark was reproduced in small detached pieces.

I have endeavoured to prove in former communications,* that the true sap of trees acquires those properties which distinguish it from the fluid recently absorbed, by circulating through the leaf; and that it descends down the bark, where part of it is employed in generating the new substances annually added to the tree; and that the remainder, not thus expended, passes into the alburnum, and there joins the ascending current of sap. The cellular substance, both of the bark and alburnum, has been proved, in the preceding experiments, to be capable of affording the sap a passage through it; and therefore it appears not very improbable, that it executes an office similar to that of the anastomosing vessels of the animal economy, when the cellular surfaces of the bark and alburnum are in contact with each other; and, when detached, it may be inferred, that the passing fluid will exude from both surfaces: because almost all the vessels of trees appear to be capable of an inverted action in giving motion to the fluids which they carry.

As the power of generating a new bark appeared in the preceding cases to exist alike in the sap of the bark and of the alburnum, I was anxious to discover how far the fluid, which ascends through the central vessels of the succulent annual shoot, is endued with similar powers. Having therefore made two circular incisions through the bark, round the stems of several annual shoots of the vine, as early in the summer as the alburnum within them had acquired sufficient

* Phil. Trans. 1801, 1805, and 1806.

maturity to perform its office of carrying up the sap, I took off the bark between these incisions; and I abraded the surface of the alburnum to prevent a reproduction of it. The alburnum in the decorticated spaces soon became externally dry and lifeless; and several incisions were then made longitudinally through it. The incisions commenced a little above, and extended below the decorticated spaces, so that, if the sap of the central vessels generated a cellular substance (as I concluded it would), that substance might come into contact and form a union with the substance of the same kind emitted by the bark above and below.

The experiment succeeded perfectly, and the cellular substances generated by the central vessels, and the bark, soon united, and a perfect vascular bark was subsequently formed beneath the alburnum, and appeared perfectly to execute the office of that which had been taken off; the medulla appeared to be wholly inactive.

I have already observed, that the vessels, which were generated in the cellular substance on the surface of the alburnum of the sycamore and the apple-tree, traversed that substance in almost every direction; and the same thing appears to occur beneath the old bark, when united to the alburnum. For having attentively examined through every part of the spring and summer, the formation of the internal bark, and alburnous layer beneath it, round the bases of regenerated buds, which I had made to spring from smooth spaces on the roots and stems of trees, I found every appearance perfectly consistent with the preceding observations. A single shoot only was suffered to spring from each root and stem, and from the base of this, in every instance the cortical vessels

dispersed themselves in different directions. Some descended perpendicularly downwards, whilst others diverged on each side, round the alburnum, with more or less inclination downwards, and met on the opposite side of it. The same pulpous and cellular substance appeared to cover the surfaces of the bark and alburnum, when in contact with each other, as when detached; and through this substance the ramifications of the vessels of the new bark extended themselves, appearing to receive their direction from the fluid sap which descended from the bark of the young shoots, and not to be, in any degree, influenced in their course by the direction taken by the cortical and alburnous vessels of the preceding year.

Whenever the vessels of the bark, which proceeded from different points, met each other, an interwoven texture was produced, and the alburnum beneath acquired a similar organisation: and the same thing occurs, and is productive of very important effects, in the ordinary course of the growth of trees. The bark of the principal stem, and of every lateral branch, contains very numerous vessels, which are charged with the descending true sap; and at the juncture of the lateral branch with the stem, these vessels meet each other. A kind of pedestal of alburnum, the texture of which is much interwoven, is in consequence formed round the base of the lateral branch; which thus becomes firmly united to the tree. This pedestal, though apparently a part of the branch, derives a large portion of the matter, annually added to it, from the cortical vessels of the principal stem; and thence, in the event of the death of the lateral branch, it always continues to live. But it not unfrequently happens, that a lateral branch forms a very acute angle with the principal stem, and, in this case,

the bark between them becomes compressed and inactive; no pedestal is in consequence formed, and the attachment of such a branch to the stem becomes extremely feeble and insecure.* Instead of the reproduced buds of the preceding experiment, buds were inserted in the foregoing summer, or attached by grafting in the spring; and, when these succeeded, though they were in many instances taken from trees of different species, and even of different genera, no sensible difference existed in the vessels, which appeared to diverge into the bark of the stock, from these buds and from those reproduced in the preceding experiments.

It appears, therefore, probable, that a pulpous organisable mass first derives its matter either from the bark, or the alburnum; and that this matter subsequently forms the new layer of bark; for, if the vessels had proceeded, as radicles,† from the inserted buds, or grafts, such vessels would have been, in some degree, different from the natural vessels of the bark of the stocks; and it does not appear probable, even without referring to the preceding facts, that vessels should be extended, in a few days, by parts successively added to their

* The advantages, which may be obtained by pruning timber trees judiciously, appear to be very little known. I have endeavoured to ascertain the practicability of giving to trees such forms as will render their timber more advantageously convertible to naval or other purposes. The success of the experiments, on small trees has been complete, and the results perfectly consistent, in every case, with the theory I have endeavoured to support in former memoirs; and I am confident, that by appropriate management, the trunks and branches of growing trees may be moulded into the various forms best adapted to the use of the ship-builder; and that the growth of the trees may at the same time be rendered considerably more rapid, without any expense or temporary loss to the proprietor.

† Darwin's Phytologia.

extremities, from the leaves to the extremities of the roots; which are, in many instances, more than two hundred feet distant from each other. I am, therefore, inclined to believe, that, as the preceding facts seem to indicate, the matter, which composes the new bark, acquires an organisation calculated to transmit the true sap towards the roots, as that fluid progressively descends from the leaves in the spring; but whether the matter, which enters into the composition of the new bark, be derived from the bark or the alburnum, in the ordinary course of the growth of the tree, it will be extremely difficult to ascertain.

It is, however, no difficult task to prove, that the bark does not, in all cases, spring from the alburnum; for many cases may be adduced in which it is always generated previously to the existence of the alburnum beneath it: but none, I believe, in which the external surface of the alburnum exists previously to the bark in contact with it, except when the cortical substance has been taken off, as in the preceding experiments. In the radicle of germinating seeds, the cortical vessels elongate, and new portions of bark are successively added to their points, many days before any alburnous substance is generated in them; and in the succulent annual shoot the formation of the bark long precedes that of the alburnum. In the radicle the sap appears also evidently to descend* through the cortical vessels,† and in the succulent annual shoot it as evidently passes up through the central

* Phil. Trans. 1805 and 1806.

† I wish it to be understood, that I exclude in these remarks, and in those contained in my former Memoirs, all trees of the palm kind, with the organisation of which I am almost wholly unacquainted.

vessels,* which surround the medulla. In both cases a cellular substance, similar to that which was generated in the preceding experiments, is first formed, and this cellular substance in the same manner subsequently becomes vascular; whence it appears, that the true sap, or blood of the plant, produces similar effects, and passes through similar stages of organisation, when it flows from different sources, and that the power of generating a new bark, properly speaking, belongs neither to the bark nor alburnum, but to a fluid, which pervades alike the vessels of both.

I shall, therefore, not attempt to decide on the merits of the theory of MALPIGHI, or of HALES, respecting the reproduction of the interior bark; but I cannot by any means admit the hypothesis of MALPIGHI and other naturalists, relative to the transmutation of bark into alburnum; and I propose to state my reasons for rejecting that hypothesis, in the next communication I have the honour to address to you.

I am, my dear Sir,

Your much obliged obedient Servant,

T. A. KNIGHT.

Elton, Dec. 18, 1806.

* Phil. Trans. 1805. MIRBEL has called the tubes, which I call the central vessels, the "tissu tubulaire" of the medulla.

VI. *An Investigation of the general Term of an important Series in the inverse Method of finite Differences. By the Rev.* John Brinkley, *D. D. F. R. S. and* Andrews *Professor of Astronomy in the University of Dublin. Communicated by the Astronomer Royal.*

Read February 26, 1807.

THE theorems relative to finite differences, given by M. LAGRANGE in the Berlin Memoirs for 1772, have much engaged the attention of mathematicians. M. LAPLACE has been particularly successful in his investigations respecting them; yet an important difficulty remained, to endeavour to surmount which is the principal object of this Paper. The theorems alluded to may be thus stated.

Let u represent any function of x. Let $x+h$, $x+2h$, $x+3h$, &c. be successive values of x, and u_1, u_2, u_3 &c. corresponding successive values of u. Let $\Delta^n u$ represent the first term of the nth order of differences of the quantities u, u_1, u_2 &c. And let also $S^n u$ represent the first term of a series of quantities, of which the first term of the nth order of differences is u. Then (e representing the series $1+1+\frac{1}{1.2}+\frac{1}{1.2.3}+$, &c.)

1. $\Delta^n u=(e^{\frac{\dot{u}}{\dot{x}}b}-1)^n$ 2. $S^n u=(e^{\frac{\dot{u}}{\dot{x}}b}-1)^{-n}$ provided that in the expansion of $(e^{\frac{\dot{u}}{\dot{x}}b}-1)^n$, $\frac{\dot{u}^2}{\dot{x}^2}$, $\frac{\dot{u}^3}{\dot{x}^3}$ &c. be substituted for

$\left(\frac{\dot{u}}{\dot{x}}\right)^2$, $\left(\frac{\dot{u}}{\dot{x}}\right)^3$ &c.; and provided that in the expansion of $\left(e^{\frac{\dot{u}}{\dot{x}}h}-1\right)^{-n}$, $\text{fl.}^n u\dot{x}^n$, $\text{fl.}^{n-1} u\dot{x}^{n-1}$, &c. be substituted for $\left(\frac{\dot{u}}{\dot{x}}\right)^{-n}$, $\left(\frac{\dot{u}}{\dot{x}}\right)^{-n+1}$ &c. and $\frac{\overset{2}{\dot{u}}}{\dot{x}^2}$, $\frac{\overset{3}{\dot{u}}}{\dot{x}^3}$, &c. be substituted for $\left(\frac{\dot{u}}{\dot{x}}\right)^2$, $\left(\frac{\dot{u}}{\dot{x}}\right)^3$, &c.

These theorems, which M. LAGRANGE had not demonstrated except by induction, have since been accurately investigated in different ways by M. LAPLACE,* and also by M. ARBOGAST†.

The expanded formula for $S^n u$, or, more accurately speaking, the natural series for $S^n u$ is of the form

$$\frac{\alpha}{b^n}\text{fl.}^n u\dot{x}^n + \frac{\beta}{b^{n-1}}\text{fl.}^{n-1} u\dot{x}^{n-1} \ldots . \frac{u}{b}\text{fl.}\, u\dot{x} + \nu u + \pi\frac{\dot{u}}{\dot{x}}h + \rho\frac{\ddot{u}}{\dot{x}^2}h^2 + \&c.$$

The coefficients α, β, γ, &c. are readily obtained by equations of relation, which were first given by LAGRANGE. But to complete the solution it is obviously necessary to obtain the law of progression, and be able to ascertain any coefficient independent of the preceding ones. This has not hitherto been done, as far as I know, except in the case of $n=1$. M. LAPLACE has given a very ingenious investigation for that case.‡ This has been copied with just encomiums by M. LACROIX,§ who does not mention that any one had accomplished it for any other value of n. In this Paper the general term is given for any value of n, the law of which is remarkably simple. A particular formula, remarkably simple as to its law, is also given for the case of $n=1$, from which that of LAPLACE may be deduced.

* *Mém. Acad. Scien.* 1777 and 1779.

† ARBOGAST *du Calcul des Derivations*, Art. 395, &c.

‡ *Mém. Acad. Scien.* 1777.

§ LACROIX *Traité des Differences et des Series*, Art. 918.

In the case of $n=1$ and $h=1$ the formula for Su is of the form α fl. $u\dot{x} + \beta u + \gamma \frac{\dot{u}}{\dot{x}} + \delta \frac{\ddot{u}}{\dot{x}^2} +$ &c. and exhibits the sum of a series of which u any function of x is the general term. For which purpose it was first given by EULER in the VIth Vol. *Com. Petropol.* and afterwards demonstrated in the VIIIth Vol. of the same work. It has been differently investigated since by several authors.* But LAPLACE appears to have been the first who gave a general term for the coefficients. EULER seems to have sought it in vain, for he says "Ipsa series "coefficientum α, β, γ &c. ita est comparata ut vix credam "pro eâ terminum generalem posse exhiberi."

As preparatory to the main object of this enquiry it has been thought proper to give investigations of the above two theorems, which will probably be found as simple as any that have appeared. It has also been found necessary, for avoiding very complex formula, to adopt a peculiar notation, which requires some explanation.

Notation.

The first term of the nth order of differences of the series 0^m, 1^m, 2^m, &c. is denoted by - - - - $\Delta^n 0^m$

$\frac{x^n}{1.2.3..n}$ is denoted by - - - - - - $\underline{x^n}$

$\frac{\dot{x}^n}{1.2.3..n}$ is denoted by - - - - - - $\underline{\dot{x}^n}$

$\frac{\Delta^n 0^m}{1.2.3..n.1.2.3..m}$ is denoted by - - - - - $\underline{\Delta^n}\ \underline{0^m}$

which omission of the denominators cannot produce any inconvenience, because the indices sufficiently point them out whether those indices refer to powers, fluxions, or differences.

* MACLAURIN'S Fluxions, Vol. II. p. 672, &c. WARING'S Med. Analyt. p. 581, &c.

$\frac{1}{1.2.3..n}$ is denoted by - - - - - $\underline{1^n}$

$\frac{1}{(1.2.3..n)^m\ 1\ 2.3..m}$ is denoted by - - - - $\underline{(1^n)^m}$

According to which notation,

$e = 1 + 1 + \frac{1}{1.2} + \frac{1}{1.2.3} + \&c. = 1 + 1 + \underline{1^2} + \underline{1^3} + \&c.$

$e^x = 1 + x + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} + \&c. = 1 + x + \underline{x^2} + \underline{x^3} + \&c.$

If the quantity $\underline{1^p}^t\ \underline{1^q}^v$ &c. or $\frac{1}{(1.2..p)^t(1.2..q)^v\,1.2..t.1.2..v\ \&c.}$ has various values arising from different values of p, q, t, v, &c. then the sum of them all is denoted by $\int \underline{1^p}^t\ \underline{1^q}^v$ &c.

Theorem I. Let u be a function of x and $x+h$, $x+2h$,--- $x+nh$ successive values of x. Then $\Delta^n u = (e^{\frac{\dot{u}}{\dot{x}}b} - 1)^n$ if after the expansion of this latter quantity $\frac{\ddot{u}}{\dot{x}^2}, \frac{\dddot{u}}{\dot{x}^3}$, &c. be substituted for $\left(\frac{\dot{u}}{\dot{x}}\right)^2, \left(\frac{\dot{u}}{\dot{x}}\right)^3$, &c. respectively.

Demonstration. Let the successive values of u be represented by ${}_1u, {}_2u, {}_3u, \ldots {}_{(n-1)}u, {}_nu$. Then by TAYLOR's theorem,

${}_nu = u + \frac{\dot{u}}{\dot{x}}nh + \frac{\ddot{u}}{\dot{x}^2}\underline{n^2}h^2 + \frac{\dddot{u}}{\dot{x}^3}\underline{n^3}h^3 + \&c. = u + e^{\frac{\dot{u}}{\dot{x}}nb} - 1$, substituting as above-mentioned. In like manner,

${}_{(n-1)}u = u + e^{\frac{\dot{u}}{\dot{x}}(n-1)b} - 1$, substituting &c.

${}_{(n-2)}u = u + e^{\frac{\dot{u}}{\dot{x}}(n-2)b} - 1$ &c. &c.

${}_1u = u + e^{\frac{\dot{u}}{\dot{x}}b} - 1$ &c. &c.

$u = u + 1 - 1.$

Hence $u-1$ being common to each term, we have by the differential theorem,

$\Delta^n u = e^{\frac{\dot{u}}{\dot{x}} nb} - ne^{\frac{\dot{u}}{\dot{x}}(n-1)b} + n\left(\frac{n-1}{2}\right) e^{\frac{\dot{u}}{\dot{x}}(n-2)b} - \&c. \pm 1 = (e^{\frac{\dot{u}}{\dot{x}} b} - 1)^n$ substituting as above-mentioned.

Theorem II. $\Delta^n u = \frac{\overset{n}{\dot{u}}}{\dot{x}^n} h^n + a\frac{\overset{n+1}{\dot{u}}}{\dot{x}^{n+1}} h^{n+1} + b\frac{\overset{n+2}{\dot{u}}}{\dot{x}^{n+2}} h^{n+2} - \&c.$

$M\frac{\overset{n+m}{\dot{u}}}{\dot{x}^{n+m}} h^{n+m} + \&c.$ where M is the coeff. of h^{n+m} in the expansion of $(e^b - 1)^n$ or of h^m in the expansion of $\left(\frac{e^b-1}{b}\right)^n$ or of $(1 + \frac{b}{1.2} + \frac{b^2}{1.2.3} + \&c.)^n$

Dem. By *Theor.* I. $\Delta^n u = (e^{\frac{\ddot{u}}{\dot{x}} b} - 1)^n$ making the necessary change after expansion. This change does not affect the numerical coefficients, which are evidently the same as those of $(e^b - 1)^n$, and because $(e^b - 1)^n = h^n (1 + \frac{b}{1.2} + \frac{b^2}{1.2.3} + \&c.)^n$ the theorem is manifest.

Theorem III. The coefficient of $\frac{\overset{n+m}{\dot{u}}}{\dot{x}^{n+m}} h^{n+m} = \Delta^n \underline{o}^{n+m}$.

Dem. The successive values of u are

$u,\ u + \frac{\dot{u}}{\dot{x}} h + \frac{\overset{2}{\dot{u}}}{\dot{x}^2} \underline{h^2} + \&c.,\ u + \frac{\dot{u}}{\dot{x}} 2h + \frac{\overset{2}{\dot{u}}}{\dot{x}^2} 2^2 \underline{h^2} + \&c.,\ u + \frac{\dot{u}}{\dot{x}} 3h + \frac{\overset{2}{\dot{u}}}{\dot{x}^2} 3^2 \underline{h^2} + \&c.$ and therefore (the whole nth differences being made up of the nth difference of the parts) we have

$\Delta^n u = \frac{\dot{u}}{\dot{x}} h \Delta^n o + \frac{\overset{2}{\dot{u}}}{\dot{x}^2} h^2 \Delta^n \underline{o}^2 + \text{---} \frac{\overset{n+m}{\dot{u}}}{\dot{x}^{n+m}} h^{n+m} \Delta^n \underline{o}^{n+m} + \&c.$

where it is to be observed that whilst $n+m$ is less than n $\Delta^n \underline{o}^{n+m} = o$, and $\Delta^n \underline{o}^n = 1$, as is well known, and easily appears from *Theorem* II.

Theorems relative to the inverse Method of finite Differences.

Theorem IV. Let u be a function of x and $x+h$, $x+2h$, &c. successive values of x, as before, then

$S^n u = (e^{\frac{\dot{u}}{\dot{x}}h} - 1)^{-n}$ if after expansion $\left(\frac{\dot{u}}{\dot{x}}\right)^{-n}$, $\left(\frac{\dot{u}}{\dot{x}}\right)^{-n+1}$ &c. are changed into fl.^{n} $u\dot{x}^n$, fl.$^{n-1}$ $u\dot{x}^{n-1}$, &c. and $\left(\frac{\dot{u}}{\dot{x}}\right)^2$, $\left(\frac{\dot{u}}{\dot{x}}\right)^3$, &c. into $\frac{\ddot{u}}{\dot{x}^2}$, $\frac{\overset{3}{\dot{u}}}{\dot{x}^3}$, &c.

Demonstration. By *Theorem* II.

$$(1)\text{---}u = A\frac{\overset{n}{\dot{(S^n u)}}}{\dot{x}^n}h^n + B\frac{\overset{n+1}{\dot{(S^n u)}}}{\dot{x}^{n+1}}h^{n+1} + C\frac{\overset{n+2}{\dot{(S^n u)}}}{\dot{x}^{n+2}}h^{n+2} + \text{ &c.}$$

where A, B, C, &c. are the coefficients of 1, h, h^2, &c. in the expansion of $\left(\frac{e^b-1}{b}\right)^n = (1 + \frac{b}{1.2} + \frac{b^2}{1.2.3} +, \text{ &c.})^n$. So that if $v = (1+h+\underline{h^2}+ \text{ &c.})^n$, making $h=0$, $v=A$, $\dot{v}=B\dot{h}$, $\underline{\ddot{v}}=C\dot{h}^2$ &c.

$$(2)\text{---Let}^*\frac{\overset{n}{\dot{(S^n u)}}}{\dot{x}^n}h^n = \alpha u + \beta\frac{\dot{u}}{\dot{x}}h + \gamma\frac{\overset{2}{\dot{u}}}{\dot{x}^2}h^2 + \delta\frac{\overset{3}{\dot{u}}}{\dot{x}^3}h^3 + \text{ &c.}$$ It is evident that this assumption may be made to satisfy the equation (1).

Then, by taking the successive fluxions of equation (2) and substituting in equation (1), we have

* Euler uses a similar assumption in his investigation of the sum of a series from its general term, p. 15, Tom. VIII. *Com. Petropol.*

$$u = \begin{cases} A\alpha u + A\beta \frac{\dot{u}}{\dot{x}} h + A\gamma \frac{\overset{2}{\dot{u}}}{\dot{x}^2} h^2 + A\delta \frac{\overset{3}{\dot{u}}}{\dot{x}^3} h^3 + \text{\&c.} \\ \qquad B\alpha \frac{\dot{u}}{\dot{x}} h + B\beta \frac{\overset{2}{\dot{u}}}{\dot{x}^2} h^2 + B\gamma \frac{\overset{3}{\dot{u}}}{\dot{x}^3} h^3 + \text{\&c.} \\ \qquad\qquad + C\alpha \frac{\overset{2}{\dot{u}}}{\dot{x}^2} h^2 + C\beta \frac{\overset{3}{\dot{u}}}{\dot{x}^3} h^3 + \text{\&c.} \\ \qquad\qquad\qquad D\alpha \frac{\overset{3}{\dot{u}}}{\dot{x}^3} h^3 + \text{\&c.} \\ \qquad\qquad\qquad\quad \text{\&c.} \qquad \text{\&c.} \end{cases}$$

Hence

$$\begin{aligned} &A\alpha = 1 \\ &A\beta + B\alpha = 0 \\ &A\gamma + B\beta + C\alpha = 0 \qquad (3) \\ &A\delta + B\gamma + C\beta + D\alpha = 0 \\ &\&c. \qquad \&c. \qquad \&c. \end{aligned}$$

In order to obtain the values of α, β, γ, δ, &c. let z represent a function of h which expanded gives $\alpha + \beta h + \gamma h^2 + \delta h^3 + \text{\&c.}$ or $z = \alpha + \beta h + \gamma h^2 + \delta h^3 + \text{\&c.}$

Then by equations (3) we have, when $h = 0$

(4) --- $\underline{v}\overset{m}{\dot{z}} + \underline{\dot{v}}\overset{m-1}{\dot{z}} + \underline{\ddot{v}}\overset{m-2}{\dot{z}} + \text{---} \underline{\dot{v}}\overset{m-1}{\dot{z}} + \underline{\dot{v}}\overset{m}{z} = 0$, m denoting any number. For taking m successively 1, 2, 3, &c. and dividing by $\dot{h}$, $\dot{h}^2$, $\dot{h}^3$, &c. we obtain the equations (3). Now the equation (4) is the mth fluxion of

(5) --- vz = constant, divided by $1.2.3 \ldots m$. When $h=0$ $vz = A\alpha = 1$, hence const. $= 1$, therefore $vz = 1$, or $z = \frac{1}{v} = \left(\frac{h}{e^h - 1}\right)^{-n} = \left(1 + \frac{h}{1.2} + \frac{h^2}{1.2.3} + \text{\&c.}\right)^{-n}$ *

* It is hoped, that this investigation of the value of z is stated with sufficient clearness. It is a very simple example of a method of very extensive application with respect to analytical functions. The theorems for finding fluxions *per saltum*, communicated by me to the Royal Irish Academy, in November, 1798, and published

Let the equation (2) be multiplied by $\frac{\dot{x}^n}{h^n}$, and the nth fluent taken, and we have

$$S^n u = \frac{\alpha}{h^n} \text{fl.}^n u\dot{x}^n + \frac{\beta}{h^{n-1}} \text{fl.}^{n-1} u\dot{x}^{n-1} + \ldots \frac{\mu}{h} \text{fl. } u\dot{x} + \nu u +$$

$$\pi \frac{\dot{u}}{\dot{x}} h + \rho \frac{\ddot{u}}{\dot{x}^2} h^2 \text{ \&c.}$$

Also from the above value of z, it is easy to see that the expansion of $\left(e^{\frac{\dot{u}}{\dot{x}} h} - 1 \right)^{-n} = \frac{1}{h^n} \left(\frac{h}{e^{\frac{\dot{u}}{\dot{x}} h} - 1} \right)^n$ gives

in the seventh volume of their Transactions, furnish a general method of reducing any function of x to a series ascending by the powers of x, and that either by assigning at once the coefficient of x^n, or by equations of relation between the coefficients. By the *converse* of that method we are enabled, either from the general coefficient, or from the equations of relation, to arrive at the primitive function. The converse, therefore, applies to the summation of series, to the investigation of the general term of a recurring series, and to several other important purposes. It is evident, that it applies to finding the general term of a recurring series, because from the given scale of relation, the primitive function can be deduced, and from the primitive function, the general coefficient may be determined. The same method extends to the reduction of any function of x, y, z, &c. and therefore the converse to finding the general term of double, triple, &c. recurring series.

I had not considered the converse of the method of reduction of analytical functions afforded by my theorems for finding fluxions *per saltum*, till I had seen M. Arbogast's ingenious work, entitled "Du Calcul des Derivations." As those theorems furnish every thing that is given in the former part of his treatise, and likewise admit of more extensive application; so also the converse of them serve for deducing, with greater facility, every thing respecting recurring series, &c. contained in the same Treatise.

The important uses to be derived from finding fluxions per saltum in the reduction of analytical functions, and from the converse, induced me to draw up a particular work on that subject. Its publication has hitherto been delayed by my unwillingness to offer a fluxional notation different from either that of Newton or Leibnitz, each of which is very inconvenient as far as regards the application of the theorems for finding fluxions *per saltum*.

$\frac{\alpha}{b^n}\left(\frac{\dot{u}}{\dot{x}}\right)^{-n}+\frac{\beta}{b^{n-1}}\left(\frac{\dot{u}}{\dot{x}}\right)^{-n+1}+\ldots\frac{\mu}{b}\left(\frac{\dot{u}}{\dot{x}}\right)^{-1}+\nu u+\pi\left(\frac{\dot{u}}{\dot{x}}\right)h+$
$\rho\left(\frac{\dot{u}}{\dot{x}}\right)^2 h^2+$ &c.

Whence the truth of the theorem is manifest.

Theorem V. $S^n u = \left(\frac{\alpha}{b^n}\right)$ fl.$^n u\dot{x}^n+\frac{\beta}{b^{n-1}}$ fl.$^{n-1} u\dot{x}^{n-1}+$ .. &c.

where the numerical coefficient of the $m+1$ term is the coefficient of h^m in the expansion of $\left(1+\frac{b}{1.2}+\frac{b^2}{1.2.3}+\text{&c.}\right)^{-n}$.

The demonstration of this is contained in that of the last theorem.

Previously to the investigation of the numerical coefficient of the general $m+1$ term, it will be convenient to premise the following lemmas.

Lemma I. Let n represent any affirmative integral number, and m any other affirmative integral number not greater than n. Then

$\int\left(\underline{1^p}\right)^t\left(\underline{1^q}\right)^v\left(\underline{1^r}\right)^w$ &c. $=\underline{\Delta}^m\,\underline{o}^n$ where p, q, r, &c. $: t, v, w$, &c. represent any affirmative integral numbers satisfying the equations $t+v+w+$ &c. $= m$

$tp+vq+wr+$ &c. $= n$.

Demonstration.

Let $a^{(1)} = \underline{1}^n$

$a^{(2)} = 1.2\int\left(\underline{1^p}\right)^t\left(\underline{1^q}\right)^v$ where $t+v=2$ & $tp+vq=n$.

$a^{(3)}=1.2.3\int\left(\underline{1^p}\right)^t\left(\underline{1^q}\right)^v\left(\underline{1^r}\right)^w$ where $t+v+w=3$ and $tp+vq+rw=n$.

. . . . . .

. . . . . .

$a^{(m)}=1.2.3..m\int\left(\underline{1^p}\right)\left(\underline{1^q}\right)^v\left(\underline{1^r}\right)^w$ &c. where $t+v+w+$ &c. $=m$ and $tp+vq+rw+$ &c. $=n$.

Then we shall find that

$$(1) \text{ --- } \underline{m}^n = m\,a^{(1)} + \frac{m(m-1)}{1.2}\,a^{(2)} + \frac{m(m-1)(m-2)}{1.2.3}\,a^{(3)} + \ldots \\ ma^{(m-1)} + a^{(m)}$$

For taking m quantities α, β, γ, &c. we easily deduce by help of the multinomial theorem,

$$\underline{(\alpha+\beta+\gamma+\text{ &c.})}^n = \underline{\alpha}^n + \underline{\beta}^n + \text{ &c.} + \int \underline{\alpha}^p\,\underline{\beta}^q + \text{ &c.} + \\ \int \underline{\alpha}^p\,\underline{\beta}^q\,\underline{\gamma}^r + \text{ &c.} + \text{ &c.}$$

where $p+q=n$, $p+q+r=n$, &c. &c.

Now if we consider a term $\int \underline{\alpha}^p\,\underline{\beta}^q\,\underline{\gamma}^r$ &c. where k quantities α, β, γ, &c. are concerned, and if $p', p' \ldots$ (t' numbers), $q', q' \ldots$ (v' numbers), $r', r' \ldots$ (w' numbers), denote values of p, q, r, &c. satisfying the equation $p+q+r+$ &c. (k terms) $=n$, we shall see, that the number of the terms, in which these values $p', p', \ldots q', q', \ldots r', r'$, &c. are found, is the number of combination of m things, taking k together into the number of permutations of k things, of which t', v', &c. are the same. This is evident, because in any product $\alpha\beta\gamma$ (k factors), the indices $p', p', \ldots q', q'$, &c. are to be annexed in every possible order. And when the quantities α, β, γ, &c. are each units, each of the quantities is expressed by the same quantity, $1^{t'} \cdot 1^{t'} \ldots 1^{q'} \cdot 1^{q'} \ldots 1^{r'} \cdot 1^{r'}$ &c. therefore the sum of all of them is

$$\frac{m(m-1)\ldots(m-k+1)}{1.2\ldots k} \times \frac{1.2\ldots k}{1.2\ldots t'.1\ 2\ldots v' \text{ &c.}} \int \left(\underline{1^{p'}}\right)^{t'} \left(\underline{1^{q'}}\right)^{v'} \text{ &c.}$$

Hence, when α, β, γ, &c. are each unity,

$$\int \underline{\alpha}^p\,\underline{\beta}^q\,\underline{\gamma}^r \text{ &c.} + \text{ &c. } (k \text{ quantities}) = \frac{m(m-1)\ \ldots\ (m-k+1)}{1.2\ldots k}\,a^{(k)}$$

Hence by substituting for k the numbers 1, 2, 3, &c. we easily obtain the equation (1). From which, substituting for m successively 1, 2, 3, &c. we obtain

$$\underline{1}^n = a^{(1)}$$
$$\underline{2}^n = 2a^{(1)} + a^{(2)}$$
$$\underline{3}^n = 3a^{(1)} + 3a^{(2)} + a^{(3)}$$
. . . . . .
. . . . . .
$$\underline{m}^n = ma^{(1)} + \frac{m(m-1)}{1.2}a^{(2)} \ldots\ldots ma^{(m-1)} + a^{(m)}$$

These equations correspond to the equations between the terms of a series of quantities, and the first terms of their respective orders of differences, i. e. $\underline{0}^n$, $\underline{1}^n$, $\underline{2}^n$, &c. correspond to the series, and $a^{(1)}$, $a^{(2)}$, &c. to the respective orders of differences.

Hence we conclude, that $a^{(m)} = \Delta^m \underline{0}^n$, and therefore that $\int \left(\underline{1}^p\right)^t \left(\underline{1}^q\right)$ &c. $= \underline{\Delta}^m \underline{0}^n$.

Lemma II. Every thing being, as in the preceding lemma, except that unity is excluded from the values of p, q, r, &c.

$$\int \left(\underline{1}^p\right)^t \left(\underline{1}^q\right)^v \left(\underline{1}^r\right)^w \text{ &c.} = \underline{\Delta}^m \underline{0}^n - \underline{\Delta}^{m-1} \underline{0}^{n-1} + \frac{1}{1.2} \underline{\Delta}^{m-2} \underline{0}^{n-2} - \frac{1}{1.2.3} \underline{\Delta}^{m-3} \underline{0}^{n-3} + \ldots \text{(to } m \text{ terms.)}$$

Demonstration. Let $b^{(m)}$, $b^{(m-1)}$, $b^{(m-2)}$, &c, represent $1.2\ldots m \int \left(\underline{1}^p\right)^t \left(\underline{1}^q\right)^v$ &c.; $1.2\ldots(m-1) \int \left(\underline{1}^p\right)^t \left(\underline{1}^q\right)^v$ &c.; &c. &c. respectively: these latter quantities being defined, as in the preceding lemma, except that unity is excluded from among the values of p, q, &c.

Then it is easy to see, if $a^{(m)}$, $a^{(m-1)}$, &c. denote as in the preceding lemma,

that $a^{(m)} = b^{(m)} + mb^{(m-1)} + \frac{m(m-1)}{1.2} b^{(m-2)} +$ &c.

that $a^{(m-1)} = b^{(m-1)} + (m-1)\, b^{(m-2)} + \frac{(m-1)(m-2)}{1.2}$ $b^{(m-3)}$ + &c.

. . . . . .

. . . . . .

&c. &c.

These equations are the same as the equations between the terms of a series of quantities, and the first terms of their respective orders of differences;

. . . $a^{(m-1)}$, $a^{(m)}$, corresponding to the terms of the series, and . . . $b^{(m-1)}$, $b^{(m)}$, to the first terms of the respective orders of differences.

Whence we conclude, that $b^{(m)} = a^{(m)} - ma^{(m-1)} + \frac{m(m-1)}{1.2}$ $a^{(m-2)}$ — &c. = (by the preceding lemma) $= \Delta^m \underline{o}^n - m\Delta^{m-1} \underline{o}^{n-1} + \frac{m(m-1)}{1.2} \Delta^{m-2} \underline{o}^{n-2}$ — &c. and therefore, excluding unity from the values of p, q, &c.

$\int \left(\underline{1}^p\right)^t \left(\underline{1}^q\right)^v \left(\underline{1}^r\right)^w$ &c. $= \underline{\Delta}^m \underline{o}^n - \underline{\Delta}^{m-1} \underline{o}^{n-1} + \frac{1}{1.2} \underline{\Delta}^{m-2} \underline{o}^{n-2}$

. . . . (m terms).

Theorem VI. Let $S^n u = \frac{1}{b^n}$ fl. $^n u \dot{x}^n + \frac{c^{(1)}}{p^{n-1}}$ fl.$^{n-1}$ $u \dot{x}^{n-1}$ + &c. (*vid. Theor.* V.), then $c^{(m)}$ or the coefficient of the $m+1$ term $= -n\left(\frac{n+2}{1}\right)\left(\frac{n+3}{2}\right) \ldots \left(\frac{n+m}{m-1}\right) \Delta^1 \underline{o}^{m+1} + n\,(n-1)\left(\frac{n+3}{1}\right)$ $\left(\frac{n+4}{2}\right) \ldots \left(\frac{n+m}{m-2}\right) \underline{\Delta}^2 \underline{o}^{m+2} - n\,(n-1)\,(n-2)\left(\frac{n+4}{1}\right)\left(\frac{n+5}{2}\right) \ldots$ $\left(\frac{n+m}{m-3}\right) \underline{\Delta}^3 \underline{o}^{m+3}$ — &c. (to m terms and m factors in each term).

Demonstration. (By *Theorem* V.) The coefficient of the

$m+1$ term is the coefficient of h^m in the expansion of $\left(1+\frac{h}{1.2}+\frac{h^2}{1.2.3}+\text{ &c.}\right)^{-n}=(1+\underline{1}^2 h+\underline{1}^3 h^2+\text{ &c.})^{-n}$.

Let $d^{(1)}=\underline{1}^{m+1}$

$d^{(2)}=\int\left(\underline{1^p}\right)^t\left(\underline{1^q}\right)^v$, where $t+v=2$, $pt+qv=m+2$, unity being excluded from the values of p, q, &c.

$d^{(3)}=\int\left(\underline{1^p}\right)^t\left(\underline{1^q}\right)^v\left(\underline{1^r}\right)^w$, where $t+v+w=3$, $pt+qv+rw=m+3$, unity &c.

. . . . . .

Then the coefficient of $h^m=-nd^{(1)}+n(n+1)d^{(2)}-n(n+1)(n+2)d..^{(3)}\pm n(n+1)..(n+m-1)d^{(m)}$.

This may easily be deduced from the multinomial Theorem, or more readily from the theorems for finding fluxions *per saltum* in the seventh volume of the Transactions of the Royal Irish Academy.

By *Lemma* II.

$d^{(1)}=\underline{\Delta^1}\,\underline{o}^{m+1}$

$d^{(2)}=\underline{\Delta^2}\,\underline{o}^{m+2}-\underline{\Delta^1}\,\underline{o}^{m+1}$

$d^{(3)}=\underline{\Delta^3}\,\underline{o}^{m+3}-\underline{\Delta^2}\,\underline{o}^{m+2}+\frac{1}{1.2}\underline{\Delta^1}\,\underline{o}^{m+1}$

. . . . . .

$d^{(m)}=\underline{\Delta^m}\,\underline{o}^{2m}-\underline{\Delta^{m-1}}\,\underline{o}^{2m-1}+\frac{1}{1.2}\underline{\Delta^{m-2}}\,\underline{o}^{2m-2}-\text{ &c.}$

Whence the coefficient of $h^m=$

n	$-\underline{\Delta^1}\,\underline{o}^{m+1}+n(n+1)$	$\underline{\Delta^2}\,\underline{o}^{m+2}+n(n+1)(n+2)$	$-\underline{\Delta^3}\,\underline{o}^{m+3}$ &c.
$n(n+1)$	$n(n+1)(n+2)$	$n(n+1)..(n+3)$	
$\frac{n(n+1)(n+2)}{1\quad 2}$	$\frac{n(n+1)..(n+3)}{1.2}$	$\frac{n(n+1)..(n+4)}{1.2}$	
(m terms)	($m-1$ terms)	($m-2$ terms)	

$=$ (by theorems for figurate numbers)

$$-n\left(\frac{n+2}{1}\right)\left(\frac{n+3}{2}\right)..\left(\frac{n+m}{m-1}\right)\Delta^1\underline{o}^{m+1}+n(n+1)\left(\frac{n+3}{1}\right)\left(\frac{n+4}{2}\right)..\left(\frac{n+m}{m-2}\right)\Delta^2\underline{o}^{m+2}-$$

$$n(n+1)(n+2)\left(\frac{n+4}{1}\right)\left(\frac{n+5}{2}\right)..\left(\frac{n+m}{m-3}\right)\Delta^3\underline{o}^{m+3}+\&c.$$ (to m terms and m factors in each term).

Cor. 1. When $n=1$ the coefficient of $h^m=$

$$=-\frac{m(m+1)}{1.2}\Delta^1\underline{o}^{m+1}+\frac{(m-1)m(m+1)}{1.2.3}\Delta^2\underline{o}^{m\pm2}\frac{(m-2)(m-1)m(m+1)}{1.2.3.4}\Delta^3\underline{o}^{m+3}\ \&c.$$

(to m terms).

Cor. 2. The same investigation holds for the coefficient of h^m in the expansion of $\left(1+\frac{b}{1.2}+\frac{b^2}{1.2.3}+\&c.\right)^n$. So that substituting $-n$ for n in the above expression we obtain by *Theorem* III.

$$\Delta^n\underline{o}^{n+m}=\pm n\left(\frac{n-2}{1}\right)\left(\frac{n-3}{2}\right)..\left(\frac{n-m}{m-1}\right)\Delta^1\underline{o}^{m+1}\mp n(n-1)\left(\frac{n-3}{1}\right)\left(\frac{n-4}{2}\right)..\left(\frac{n-m}{m-2}\right)\Delta^2\underline{o}^{m+2}\pm\&c.$$

(to m terms and m factors in each term) the upper signs taking place when m is odd, and the under when even.

This corollary furnishes an important theorem, greatly facilitating the computation of differences. It often affords a much more convenient general term for the numerical coefficients in the expression for $\Delta^n u$ than that given in *Theor.* III. *viz.* when m is small compared with n in which case the common method for the computation of $\Delta^n\underline{o}^{n+m}$ would be of little use. As in the following example:

Example. To find the first term of the $n-2$ order of differences of the series x^n, $(x+h)^n$, $(x+2h)^n$, &c.

By *Theorem* III.

$$\Delta^{n-2} x^n = n(n-1)..3x^2 h^{n-2}\ \Delta^{n-2}\underline{o}^{n-2} + n(n-1)..2xh^{n-1}\ \Delta^{n-2}\underline{o}^{n-1} + n(n-1)..1h^n\ \Delta^{n-2}\underline{o}^n$$

$$\Delta^{n-2}\underline{o}^{n-2} = 1$$

and, By this *Corollary*

$$\Delta^{n-2}\underline{o}^{n-1} = (n-2)\Delta^1\underline{o}^2 = \frac{n-2}{2}$$

$$\Delta^{n-2}\underline{o}^n = -(n-2)(n-4)\Delta^1\underline{o}^3 + (n-2)(n-3)\Delta^2\underline{o}^4 = \frac{(n-2)(3n-5)}{1.2.3.4}$$

Hence

$$\Delta^{n-2} x^n = 1.2..n\left(\frac{x^2}{1.2}h^{n-2} + \frac{n-2}{2}xh^{n-1} + \frac{(n-2)(3n-5)}{1.2.3.4}h^n\right)$$

Scholium.

The case of the above theorem, when $n=1$, has, on account of its importance, been a particular object of investigation among mathematicians. Although the formula in the first corollary is, as to its formation and law of progression, very simple, yet one more simple may be readily obtained by the joint application of a transformation given by M. LAPLACE in his method, and of the first lemma above given; which formula and its investigation are here subjoined. This and the above general formule (n being any number), as well as the formula of LAPLACE (n being 1), do not enable us to compute the successive coefficients so readily as from the equations of relation. But this circumstance, it is imagined, will not render what has been here done less worthy of the notice of mathematicians. Their researches for a general term in the case of

$n=1$, sufficiently shew of what importance the enquiry has been considered.

Theorem. $Su=\frac{1}{b}\text{fl.}\,u\dot{x}-\frac{u}{2}+a\frac{\dot{u}}{\dot{x}}h+b\frac{\overset{3}{\dot{u}}}{\dot{x}^3}h^3+\ldots P\frac{\overset{m}{\dot{u}}}{\dot{x}^m}h^m+\&c.$ in which the even powers of h are not found, and P the coeff. of $\frac{\overset{m}{\dot{u}}}{\dot{x}^m}h^m=\frac{1}{1.2.3..m(2^{m+1}-1)2^m}\Big(2^{m-1}\Delta^1 o^m-2^{m-2}\Delta^2 o^m+2^{m-3}\Delta^3 o^m\ldots.+\Delta^m o^m\Big)$.

Demonstration. The coefficient of $\frac{\overset{m}{\dot{u}}}{\dot{x}^m}h^m$, or $c^{(m+1)}$ (*Vid. Theorem* V.) is the coefficient of h^{m+1} in the expansion of $\left(1+\frac{b}{1.2}+\frac{b^2}{1.2.3}+\&c.\right)^{-1}$, or of $\left(\frac{b}{e^b-1}\right)$

1. Now $\frac{b}{e^b-1}+\frac{b}{e^{-b}-1}=-h$ . . . . . (1)

Let

$1+Ah+Bh^2\ldots..+Nh^m+Ph^{m+1}+\&c.$ { represent the expansion of $\frac{b}{e^b-1}$, m being any odd number.

Then

$-1+Ah-Bh^2\ldots..+Nh^m-Ph^{m+1}+\&c.$ { will represent the expansion of $\frac{b}{e^{-b}-1}$

Hence by equation (1) we obtain

$A+Ch^2\ldots+Nh^{m-1}+\&c.=-\frac{1}{2}$ . . . . (2)

From whence it follows that

$A=\frac{1}{2}$, $C=o\ldots.N=o$ . . . . Hence when m is any odd number the coefficient of $h^m=o$, and therefore the coefficient of $\frac{\overset{m-1}{\dot{x}}}{\dot{x}^{m-1}}h^{m-1}$.

$$2.\ \frac{b}{e^{b}-1}=\frac{\frac{1}{2}b}{e^{\frac{1}{2}b}-1}-\frac{\frac{1}{2}b}{e^{\frac{1}{2}b}+1}\ldots\ldots\ldots\ldots(3)$$

Let p, q, r, and s represent the coefficients of h^{m+1} in the expansion of $\frac{b}{e^{b}-1}$, $\frac{\frac{1}{2}b}{e^{\frac{1}{2}b}-1}$, $\frac{b}{e^{b}+1}$, and $\frac{\frac{1}{2}b}{e^{\frac{1}{2}b}+1}$ respectively: then it is easy to see that $\frac{p}{2^{m+1}}=q$, $\frac{r}{2^{m+1}}=s$ and therefore by equation (3) $p=q-s=\frac{p-r}{2^{m+1}}$, or $p=\frac{r}{1-2^{m+1}}$.

To obtain r the coefficient of h^{m+1} in the expansion of $h\left(2+h+\frac{h^2}{1.2}+\text{&c.}\right)^{-1}$

Let $d^{(1)}=\underline{1}^{m}$

$d^{(2)}=\int\left(\underline{1^{p}}\right)^{t}\left(\underline{1^{q}}\right)^{v}$ where $t+v=2$ and $pt+qv=m$

$d^{(3)}=\int\left(\underline{1^{b}}\right)^{t}\left(\underline{1^{q}}\right)^{v}\left(\underline{1^{r}}\right)^{w}$ where $t+v+w=3$ and $pt+qv+rw=m$

&c. &c. &c.

Then the coefficient of h^{m} in the expansion of $\left(2+h+\frac{h^2}{1.2}+\text{&c.}\right)^{-1}=$

$$-\tfrac{1}{2^{2}}\,d^{(1)}+\tfrac{1.2}{2^{3}}\,d^{(2)}-\tfrac{1.2.3}{2^{4}}\,d^{(3)}+\text{&c.}$$

But by *Lemma* I.

$$d^{(1)}=\Delta\,\underline{o}^{m}$$

$$1.2\,d^{(2)}=\Delta^{2}\underline{o}^{m}$$

$$1.2.3\,d^{(3)}=\Delta^{3}\underline{o}^{m}$$

&c. &c.

Hence the coefficient of h^{m+1} in the expansion of $\frac{b}{e^{b}-1}$ or co-

efficient of $\frac{u}{x^m}^m h^m$ in the expression for $Su =$

$$\frac{1}{1.2.3..m}\frac{1}{(2^{m+1}-1)2^{m+1}}\left(2^{m-1}\Delta^1 o^m - 2^{m-2}\Delta^2 o^m + \ldots\ldots + \Delta^m o^m\right)$$

The same conclusion may be derived somewhat more easily by the assistance of a diverging series, as follows. This investigation, however, is not given as affording the same satisfaction to the mind as the above demonstration,

$$\frac{1}{e^b+1} = 1 - e^b + e^{2b} - \text{ \&c.}$$

Hence it is easy to see that the coefficient of h^m

$= -\underline{1}^m + \underline{2}^m - \underline{3}^m +$ &c. in infinitum.

Also $\frac{1}{e^b+1} = \frac{e^{-b}}{1+e^{-b}} = e^{-b} - e^{-2b} + e^{-3b} -$ &c.

from which it likewise appears that the coefficient of h^m $= \pm\underline{1}^m \mp \underline{2}^m \pm \underline{3}^m \mp$ &c., the upper signs taking place when m is even, and the lower when odd. Hence when m is even $-\underline{1}^m + \underline{2}^m - \underline{3}^m +$ &c. $= \underline{1}^m - \underline{2}^m + \underline{3}^m -$ &c. and therefore necessarily $-\underline{1}^m + \underline{2}^m - \underline{3}^m +$ &c. $= 0$. Consequently, when m is even the coefficient of h^m in the expansion of $\frac{1}{e^b+1} = 0$. And generally by the appplication of a well-known theorem,

$$-\underline{1}^m + \underline{2}^m - \underline{3}^m + \text{ \&c.} = -\tfrac{1}{2}\Delta^1 \underline{o}^m + \tfrac{1}{2}\Delta^2 \underline{o}^m - \text{ \&c.}$$

Whence, &c. &c.

It may be remarked, that in the above theorem the coefficient of h^m can be computed, without using a higher quantity in the series o^m, 1^m, 2^m, &c. than $\left(\frac{m-1}{2}\right)^m$. For the first terms of the $\frac{m-1}{2}+1$, $\frac{m-1}{2}+2$, &c. orders of differences of the series $\left(\frac{m-1}{2}\right)^m$, $\left(1-\left(\frac{m-1}{2}\right)\right)^m$, $\left(2-\left(\frac{m-1}{2}\right)\right)^m$, &c. are obtained

without using a higher power than $\left(\frac{m-1}{2}\right)^m$, and thence $\Delta^{\frac{m+1}{2}} o^m \ldots . \Delta^{m-1} o^m$, and it is known, that $\Delta^m o^m = 1 \ldots m$. Thus the computation of the latter half of the terms in $\left(2^{m-1} \Delta^1 o^m - 2^{m-2} \Delta^2 o^m + \&c.\right)$ will be much facilitated.

The computation of $\Delta^{m-1} o^m$, $\Delta^{m-2} o^m$, &c. is also much facilitated by *Cor.* II. *Theorem* V.

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METEOROLOGICAL JOURNAL

for January, 1806.

1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Jan. 1	44	8	0	45	53	29,70	96	0,180	N	1	Rain.
	46	2	0	46	55	29,69	91		W	1	Cloudy.
2	36	8	0	36	52	29,70	87	0,040	W	1	Fine.
	42	2	0	42	54	29,81	84		NW	1	Cloudy.
3	31	8	0	34	52	29,83	85		S	1	Cloudy.
	40	2	0	39	53	29,72	96		S	1	Rain.
4	34	8	0	34	51	29,97	94	0,083	W	1	Fair.
	49	2	0	43	53	29,90	92		S	1	Cloudy.
5	41	8	0	41	52	29,80	90	0,105	W	1	Fair.
	46	2	0	46	53	29,91	93		WNW	2	Cloudy.
6	44	8	0	49	52	29,88	95		SW	2	Cloudy.
	53	2	0	53	54	29,85	92		SW	2	Cloudy.
7	40	8	0	40	52	30,13	94	0,043	SW	1	Fair.
	51	2	0	49	55	30,00	94		SSW	1	Rain.
8	48	8	0	48	53	29,60	93	0,070	SSW	2	Cloudy.
	50	2	0	48	55	29,50	80		SSW	2	Fair.
9	37	8	0	38	53	29,68	90		SW	2	Fair.
	49	2	0	44	54	29,72	81		W	2	Fine.
10	40	8	0	42	52	28,73	89	0,170	SW	2	Cloudy. [much wind last night.
	44	2	0	43	55	28,73	78		W	2	Fair.
11	36	8	0	39	52	29,00	87		NW	2	Fine.
	42	2	0	42	55	29,14	78		N	2	Fine.
12	36	8	0	37	51	28,65	92	0,110	E	2	Rain.
	40	2	0	40	53	28,80	92		NE	1	Cloudy.
13	33	8	0	37	51	29,55	87		NW	1	Cloudy.
	40	2	0	40	53	29,71	86		NNW	1	Fine.
14	34	8	0	42	50	29,58	94	0,105	SW	2	Cloudy.
	46	2	0	44	54	29,62	84		WNW	1	Fine.
15	40	8	0	44	51	29,38	87	0,133	WSW	2	Cloudy. [much wind last night.
	51	2	0	47	53	29,52	81		WNW	2	Fair.
16	44	8	0	50	51	29,17	92	0,030	SSW	2	Rain. [much wind last night.
	51	2	0	46	53	28,97	86		S	2	Cloudy.

METEOROLOGICAL JOURNAL
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1806	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
Jan. 17	37	8 0	38	51	29,48	91	0,095	SW	1	Cloudy.
	46	2 0	44	53	29,69	85		WNW	1	Cloudy.
18	34	8 0	38	51	29,97	86		W	1	Rain.
	50	2 0	46	53	29,84	90		S	1	Rain.
19	45	8 0	45	52	29,59	83	0,280	W	1	Cloudy.
	50	2 0	49	55	29,74	78		WNW	1	Fine.
20	45	8 0	51	53	29,61	93		SW	2	Cloudy.
	54	2 0	54	55	29,56	92		SW	2	Cloudy.
21	48	8 0	48	54	29,74	94		W	1	Fair.
	55	2 0	55	57	29,88	88		W	1	Fair.
22	45	8 0	48	55	30,02	93		SW	1	Cloudy.
	51	2 0	50	57	29,98	91		SSW	1	Cloudy.
23	49	8 0	50	55	29,77	90		SW	2	Cloudy.
	52	2 0	51	57	29,91	81		W	2	Cloudy.
24	45	8 0	50	55	29,47	94	0,075	W	2	Rain.
	50	2 0	48	58	29,54	78		WSW	2	Fair.
25	40	8 0	40	54	29,59	83		WNW	2	Fine.
	46	2 0	46	57	29,64	78		WNW	2	Fine.
26	36	8 0	37	53	29,33	87		E	1	Cloudy.
	42	2 0	42	55	29,26	87		ENE	1	Cloudy.
27	35	8 0	36	52	29,15	90	0,083	E	1	Cloudy.
	41	2 0	41	54	29,05	88		E	1	Cloudy.
28	35	8 0	36	51	29,10	93	0,235	NE	1	Cloudy.
	38	2 0	38	53	29,09	90		NE	1	Cloudy.
29	36	8 0	36	50	29,05	90		NW	1	Cloudy.
	38	2 0	37	53	29,18	87		NW	1	Snow.
30	28	8 0	29	49	29,25	90		W	1	Cloudy.
	35	2 0	35	51	29,15	87		NE	1	Cloudy.
31	30	8 0	30	48	29,31	90		SW	1	Fair.
	38	2 0	38	51	29,37	87		WNW	1	Fair.

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1806	Six's Therm. least and greatest Heat.	Time. H. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°		°	°	Inches.	°	Inches.			
Feb. 1	31	7 0	32	48	29,68	90		SW	1	Cloudy.
	39	2 0	39	50	29,83	88		NE	1	Cloudy.
2	32	7 0	33	47	29,92	90		NE	1	Snow.
	38	2 0	38	50	29,92	90		NE	1	Cloudy.
3	30	7 0	33	47	29,79	92		E	1	Cloudy.
	36	2 0	36	48	29,67	87		E	1	Cloudy.
4	32	7 0	33	46	29,52	93		E	1	Snow.
	37	2 0	37	49	29,62	90		E	1	Cloudy.
5	37	7 0	46	47	29,65	96	0,033	S	1	Cloudy.
	53	2 0	52	52	29,64	87		SSE	2	Fair.
6	41	7 0	41	48	29,64	93	0,130	SW	1	Fine.
	50	2 0	49	53	29,73	88		SSW	2	Rain.
7	46	7 0	46	50	29,59	95		S	1	Cloudy.
	53	2 0	52	54	29,62	88		S	1	Fair.
8	42	7 0	42	52	29,68	93		SSW	1	Fine.
	52	2 0	52	54	29,75	88		SW	1	Fair.
9	46	7 0	46	52	29,61	93	0,095	S	2	Cloudy. [Much wind last night.
	53	2 0	52	55	29,61	82		WSW	2	Cloudy.
10	42	7 0	42	53	29,66	92		SSW	1	Fine.
	51	2 0	51	55	29,67	85		S	1	Cloudy.
11	38	7 0	39	53	29,82	90		W	1	Cloudy.
	49	2 0	48	55	29,85	85		S	1	Fair.
12	41	7 0	41	52	29,85	90		NE	1	Cloudy.
	45	2 0	45	55	29,89	84		NW	1	Cloudy.
13	40	7 0	42	52	29,67	93	0,040	SW	1	Cloudy.
	45	2 0	44	55	29,72	82		WNW	1	Cloudy.
14	32	7 0	34	52	29,72	87		S	1	Fine.
	44	2 0	43	53	29,46	88		S	2	Rain.
15	37	7 0	40	51	29,33	92	0,052	E	1	Cloudy.
	44	2 0	44	53	29,47	87		WNW	1	Cloudy.
16	38	7 0	38	52	29,85	92		S	1	Fair.
	47	2 0	47	53	29,82	88		S	1	Cloudy.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 17	37	7	0	38	51	29,90	92	0,185	SW	1	Cloudy.
	48	2	0	47	54	30,03	83		N	1	Fair.
18	35	7	0	36	51	30,28	93		NE	1	Cloudy.
	44	2	0	44	54	30,27	82		NE	1	Cloudy.
19	39	7	0	39	51	30,21	90		NNE	1	Cloudy.
	44	2	0	44	53	30,13	82		NE	1	Cloudy.
20	40	7	0	40	51	30,04	85		NE	1	Cloudy.
	44	2	0	44	53	30,00	83		E	1	Cloudy.
21	40	7	0	43	51	29,91	90		SE	1	Cloudy.
	50	2	0	50	53	29,86	86		S	2	Cloudy.
22	47	7	0	48	52	29,63	93		S	2	Rain.
	53	2	0	53	54	29,64	76		S	2	Cloudy.
23	40	7	0	41	52	29,77	84		S	2	Cloudy.
	51	2	0	51	54	29,81	86		S	1	Cloudy.
24	46	7	0	46	54	30,12	94		SW	1	Cloudy.
	56	2	0	56	57	30,21	90		SW	1	Fair.
25	49	7	0	49	55	30,24	94		SW	1	Cloudy.
	55	2	0	55	57	30,24	87		SSW	1	Cloudy.
26	48	7	0	48	56	30,11	90		SSW	2	Cloudy.
	53	2	0	50	57	30,12	76		W	2	Fair.
27	38	7	0	43	54	29,98	85		SW	2	Cloudy.
	53	2	0	50	56	29,89	73		W	2	Fair.
28	38	7	0	38	53	30,00	81		WNW	2	Fair.
	44	2	0	44	56	30,14	72		NW	2	Fair.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Mar. 1	36	7	0	37	53	30,10	88	0,040	S	1	Rain.
	46	2	0	43	55	30,18	86		NE	1	Cloudy.
2	43	7	0	45	53	30,20	90		W	1	Cloudy.
	52	2	0	51	56	30,20	83		WNW	2	Cloudy.
3	46	7	0	49	54	29,94	83		WNW	2	Cloudy.
	54	2	0	54	56	29,94	80		WNW	2	Cloudy.
4	41	7	0	41	54	30,16	85		N	2	Rain.
	45	2	0	45	56	30,21	87		NE	2	Rain.
5	38	7	0	38	54	30,37	87		NE	1	Hazy.
	45	2	0	45	56	30,38	74		NE	1	Hazy.
6	32	7	0	33	53	30,45	83		NNE	1	Fine.
	40	2	0	40	55	30,42	78		ENE	1	Fair.
7	35	7	0	36	52	30,40	84		NE	1	Cloudy.
	49	2	0	49	55	30,33	80		NE	1	Hazy.
8	41	7	0	42	53	30,25	91		NE	1	Cloudy.
	46	2	0	46	55	30,15	86		NE	1	Cloudy.
9	40	7	0	41	52	29,78	90		SW	1	Cloudy.
	45	2	0	41	55	29,55	88		NW	1	Rain.
10	28	7	0	32	50	29,11	90	0,120	W	2	Snow.
	41	2	0	39	53	28,90	84		NW	2	Sleet.
11	33	7	0	33	50	29,05	88	0,055	WNW	2	Cloudy.
	38	2	0	38	53	29,12	83		NW	2	Cloudy.
12	28	7	0	29	49	29,13	84		NE	1	Cloudy.
	35	2	0	35	51	28,98	82		NE	1	Cloudy.
13	26	7	0	26	47	29,18	85		NW	2	Fine.
	39	2	0	39	51	29,38	77		W	2	Fair.
14	34	7	0	37	48	29,20	90		E	1	Rain.
	50	2	0	49	51	29,03	93		S	2	Rain.
15	33	7	0	33	49	29,50	86	0,100	N	1	Fair.
	43	2	0	43	53	29,57	77		NE	1	Fair.
16	35	7	0	36	49	29,26	92	0,105	E	1	Rain.
	39	2	0	39	52	29,33	93		E	1	Rain.

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1806	Six's Therm least and greatest Heat.	Time. H. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°		°	°	Inches.	°	Inches.			
Mar. 17	38	7 0	39	49	29,38	93	0,098	E	2	Rain.
	39	2 0	39	52	29,30	93		E	2	Rain.
18	39	7 0	40	50	29,31	95	0,190	NE	1	Cloudy.
	48	2 0	47	33	29,35	93		E	1	Rain.
19	38	7 0	39	50	29,40	93	0,040	SW	1	Cloudy.
	47	2 0	47	53	29,35	88		SSW	1	Cloudy.
20	42	7 0	42	51	29,21	94		NE	1	Cloudy.
	48	2 0	47	53	29,30	91		NE	1	Cloudy.
21	40	7 0	41	52	29,72	91		NW	1	Fair.
	52	2 0	52	55	29,83	77		W	1	Fair.
22	44	7 0	46	53	29,85	88		SE	1	Cloudy.
	54	2 0	53	55	29,88	87		S	2	Cloudy.
23	44	7 0	46	53	29,82	93		ESE	1	Cloudy.
	56	2 0	56	56	29,70	83		ESE	2	Fair.
24	48	7 0	49	55	29,68	93	0,105	SW	1	Cloudy.
	54	2 0	53	57	29,70	88		NE	1	Cloudy.
25	48	7 0	48	56	29,60	93	0,110	NE	1	Rain.
	53	2 0	53	58	29,60	90		N	1	Cloudy.
26	46	7 0	46	56	29,71	92	0,365	NE	1	Cloudy.
	51	2 0	51	58	29,77	87		NE	1	Cloudy.
27	46	7 0	46	56	29,85	93		NE	1	Cloudy.
	48	2 0	48	58	29,88	87		E	1	Cloudy.
28	43	7 0	43	56	29,91	87		NE	1	Cloudy.
	45	2 0	45	57	29,90	86		NE	1	Cloudy.
29	41	7 0	41	55	29,90	88		NE	2	Cloudy.
	45	2 0	45	57	29,91	85		NE	1	Cloudy.
30	40	7 0	40	55	29,91	90		NE	1	Cloudy.
	48	2 0	47	57	29,95	80		NE	1	Cloudy.
31	41	7 0	42	54	30,05	87		NE	1	Cloudy.
	48	2 0	48	57	30,10	83		ENE	1	Cloudy.

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1806	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
April 1	36	7	0	38	54	30,30	83		NE	2	Cloudy.
	44	2	0	43	56	30,35	78		NE	2	Cloudy.
2	35	7	0	38	53	30,32	82		NE	2	Cloudy.
	45	2	0	44	55	30,26	77		NE	1	Cloudy.
3	36	7	0	37	51	30,23	84		NE	2	Fair.
	44	2	0	43	54	30,25	77		NE	2	Cloudy.
4	37	7	0	38	52	30,19	85		NE	1	Cloudy.
	43	2	0	42	54	30,12	82		NE	1	Cloudy.
5	35	7	0	38	52	30,01	91	0,080	NE	1	Cloudy.
	49	2	0	48	56	29,97	80		ENE	1	Fair.
6	39	7	0	41	53	29,90	90		ENE	1	Fair.
	55	2	0	55	56	29,88	78		ENE	1	Fair.
7	41	7	0	41	53	29,90	92		ENE	1	Cloudy.
	54	2	0	54	57	29,87	79		E	1	Fair.
8	38	7	0	40	54	29,92	92		NE	1	Cloudy.
	57	2	0	57	57	29,90	87		ENE	1	Fine.
9	42	7	0	42	55	29,85	90		E	1	Cloudy.
	57	2	0	56	59	29,77	84		E	1	Fair.
10	44	7	0	45	55	29,62	92		E	2	Cloudy.
	53	2	0	51	58	29,53	85		E	1	Cloudy.
11	38	7	0	38	55	29,54	88		NE	2	Cloudy.
	44	2	0	43	56	29,57	80		NE	2	Cloudy.
12	35	7	0	36	53	29,66	76		NE	2	Cloudy.
	38	2	0	38	55	29,60	71		NE	2	Cloudy.
13	31	7	0	33	51	29,38	92		NE	2	Snow.
	38	2	0	37	53	29,40	87		NE	2	Sleet.
14	34	7	0	34	50	29,52	93		NE	2	Snow.
	43	2	0	42	52	29,61	85		NNE	2	Cloudy.
15	33	7	0	34	50	29,71	88		NNE	2	Cloudy.
	41	2	0	40	52	29,85	80		NE	2	Cloudy.
16	32	7	0	35	50	30,20	87		NNE	1	Fair.
	50	2	0	49	53	30,31	73		NE	1	Fair.

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1806	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
Apr. 17	37	7 0	40	50	30,47	84		WSW	1	Cloudy.
	55	2 0	55	54	30,45	76		W	1	Cloudy.
18	45	7 0	47	52	30,40	91		SW	1	Fair.
	58	2 0	57	56	30,36	86		SW	1	Cloudy.
19	51	7 0	51	55	30,32	94	0,165	W	1	Cloudy.
	59	2 0	58	57	30,35	93		S	1	Cloudy.
20	50	7 0	50	55	30,32	93		S	1	Cloudy.
	64	2 0	64	57	30,30	83		NNE	1	Cloudy.
21	50	7 0	51	56	30,30	92		NNE	1	Cloudy.
	59	2 0	58	58	30,27	85		NE	1	Cloudy.
22	49	7 0	50	57	30,23	90		NE	1	Cloudy.
	57	2 0	55	59	30,19	87		NE	1	Rain.
23	44	7 0	45	57	30,20	86		NE	2	Cloudy.
	50	2 0	50	57	30,24	82		NE	2	Cloudy.
24	43	7 0	45	56	30,30	85		NE	2	Cloudy.
	56	2 0	53	58	30,30	81		NE	2	Cloudy.
25	47	7 0	49	57	30,26	85		NNE	2	Cloudy.
	54	2 0	54	58	30,21	80		NNE	2	Cloudy.
26	41	7 0	42	56	30,32	82		NE	2	Cloudy.
	47	2 0	47	57	30,37	76		NE	2	Cloudy.
27	39	7 0	43	55	30,22	81		NW	1	Cloudy.
	55	2 0	55	57	30,04	78		NW	2	Cloudy.
28	43	7 0	46	55	29,79	88		NNE	1	Cloudy.
	51	2 0	49	57	29,80	77		NE	2	Cloudy.
29	38	7 0	43	55	29,74	81		SW	1	Cloudy.
	57	2 0	55	57	29,58	77		W	2	Cloudy.
30	46	7 0	48	56	29,56	79		WNW	2	Fair.
	57	2 0	54	57	29,69	77		N	2	Cloudy.

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1806	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
May 1	46	7 0	48	56	29,82	87	0,075	SE	1	Cloudy.
	57	2 0	57	58	29,80	78		S	1	Cloudy.
2	50	7 0	51	56	29,61	82		S	2	Cloudy.
	65	2 0	65	59	29,58	70		S	2	Fine.
3	50	7 0	51	58	29,63	90	0,134	NE	1	Rain.
	52	2 0	49	59	29,76	91		NE	1	Rain.
4	43	7 0	46	58	30,00	86	0,150	NE	2	Cloudy.
	54	2 0	54	58	30,08	79		NE	2	Cloudy.
5	43	7 0	46	57	30,10	87		NE	2	Cloudy.
	55	2 0	51	59	30,05	90		ENE	1	Rain.
6	45	7 0	49	58	29,86	91	0,120	NE	1	Cloudy.
	70	2 0	68	61	29,76	81		NE	1	Fine.
7	52	7 0	54	59	29,58	89	0,016	NE	1	Rain.
	72	2 0	71	62	29,56	75		ENE	1	Cloudy.
8	54	7 0	57	61	29,55	85		NE	1	Hazy.
	70	2 0	68	64	29,55	77		E	1	Fine.
9	51	7 0	53	62	29,50	87		NE	1	Cloudy.
	64	2 0	61	63	29,48	83		NE	1	Cloudy.
10	53	7 0	55	62	29,62	88		NE	1	Hazy.
	71	2 0	71	64	29,66	70		S	1	Fine.
11	52	7 0	53	63	29,76	90		NE	1	Cloudy.
	65	2 0	62	63	29,77	83		NNE	1	Fair.
12	49	7 0	51	62	29,82	87	0,105	NNE	1	Cloudy.
	59	2 0	58	62	29,81	81		NE	1	Cloudy.
13	45	7 0	48	61	29,71	97	0,190	ENE	1	Rain.
	55	2 0	54	61	29,71	95		ENE	1	Cloudy.
14	49	7 0	51	60	29,56	92		ENE	1	Rain.
	61	2 0	57	60	29,40	94		E	1	Rain.
15	52	7 0	54	60	29,44	86	0,210	S	2	Cloudy.
	64	2 0	63	61	29,60	73		SSW	2	Cloudy.
16	47	7 0	49	60	29,88	90		S	1	Cloudy.
	66	2 0	64	61	29,95	71		S	1	Fair.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
May 17	50	7	0	53	60	30,05	84		ENE	1	Fair.
	65	2	0	65	62	30,05	75		E	1	Fair.
18	51	7	0	54	60	30,26	78		NE	1	Fair.
	69	2	0	69	62	30,32	65		E	1	Fine.
19	48	7	0	53	61	30,43	74		NE	2	Fair.
	67	2	0	67	62	30,37	67		NE	2	Fine.
20	46	7	0	53	61	30,24	80		NE	2	Fine.
	72	2	0	72	64	30,17	68		ENE	2	Fine.
21	52	7	0	55	62	30,15	80		NE	2	Cloudy.
	65	2	0	63	62	30,10	73		NE	2	Cloudy.
22	51	7	0	52	62	30,13	91	0,020	NNE	2	Rain.
	70	2	0	70	64	30,16	72		NE	2	Fair.
23	46	7	0	52	62	30,28	83		NE	2	Fair.
	67	2	0	67	63	30,20	68		NE	2	Fine.
24	49	7	0	55	62	30,04	83		NE	2	Fine.
	71	2	0	71	64	29,98	68		NE	2	Fine.
25	53	7	0	58	63	29,98	80		NE	2	Fine.
	74	2	0	73	67	29,96	67		ENE	2	Fine.
26	55	7	0	60	65	29,90	80		ENE	2	Fine.
	74	2	0	73	67	29,88	70		ESE	2	Fine.
27	55	7	0	59	65	29,95	76		NE	1	Cloudy.
	70	2	0	68	65	30,00	77		ESE	1	Cloudy.
28	54	7	0	56	65	30,06	87		NE	1	Cloudy.
	75	2	0	73	66	30,04	72		NE	1	Hazy.
29	57	7	0	60	66	30,01	87		ENE	1	Cloudy.
	70	2	0	69	66	29,96	81		NE	1	Cloudy.
30	51	7	0	55	65	29,84	83		NE	1	Fair.
	67	2	0	66	66	29,77	75		NE	2	Fair.
31	47	7	0	54	64	29,87	77		NE	1	Cloudy.
	60	2	0	59	64	29,85	74		NE	1	Cloudy.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
June 1	49	7	0	54	63	29,96	80		NNE	1	Fair.
	64	2	0	63	63	29,98	76		NE	1	Cloudy.
2	54	7	0	56	63	30,1	83		NE	1	Hazy.
	68	2	0	67	64	30,14	74		NE	1	Cloudy.
3	55	7	0	56	63	30,17	78		S	2	Fair.
	73	2	0	70	64	30,10	72		SSW	1	Cloudy.
4	56	7	0	58	63	29,87	77		SSE	2	Cloudy.
	64	2	0	60	63	29,68	78		S	2	Rain.
5	51	7	0	52	62	29,60	90	0,105	WSW	1	Fair.
	68	2	0	66	63	29,64	83		SSW	1	Cloudy.
6	51	7	0	54	62	29,83	84		W	2	Fair.
	67	2	0	66	63	29,82	76		SW	2	Cloudy.
7	53	7	0	56	62	30,06	85		SW	2	Fine.
	71	2	0	71	64	30,11	73		SW	2	Fair.
8	53	7	0	56	62	30,17	83		SSE	1	Fair.
	75	2	0	75	65	30,15	75		S	2	Fair.
9	57	7	0	58	64	30,21	90		SSW	1	Hazy.
	81	2	0	81	67	30,17	72		S	1	Fine.
10	61	7	0	63	66	30,15	80		E	1	Fine.
	83	2	0	83	70	30,07	70		E	1	Fair.
11	65	7	0	65	69	30,17	83		W	1	Fair.
	75	2	0	75	70	30,28	68		N	1	Fair.
12	54	7	0	60	68	30,53	73		NE	1	Fine.
	72	2	0	72	69	30,48	67		NE	1	Fine.
13	55	7	0	59	67	30,37	53		E	1	Fine.
	77	2	0	76	69	30,27	67		SE	1	Fine.
14	56	7	0	62	68	30,18	73		ENE	1	Fine.
	82	2	0	81	71	30,15	66		NE	1	Fine.
15	63	7	0	63	67	30,25	77		E	1	Fine.
	75	2	0	74	71	30,25	68		E	2	Fine.
16	58	7	0	60	70	30,18	82		SE	1	Hazy.
	78	2	0	77	71	30,16	67		N	1	Fine.

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1806	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o					o				
June 17	54	7 0	58	68	30,15	75		NE	1	Cloudy.
	70	2 0	70	70	30,13	68		NE	1	Fair.
18	50	7 0	55	68	30,21	73		SW	1	Fine.
	68	2 0	67	69	30,26	67		NNE	1	Fine.
19	48	7 0	56	67	30,40	77		NE	1	Fair.
	71	2 0	71	68	30,40	65		NE	1	Fine.
20	54	7 0	58	68	30,44	78		S	1	Fine.
	81	2 0	80	70	30,35	66		W	1	Fine.
21	55	7 0	59	68	30,20	75		NW	1	Fine.
	72	2 0	70	68	30,14	72		NW	1	Fair.
22	56	7 0	58	68	30,07	85		NW	1	Cloudy.
	70	2 0	69	68	29,95	72		WNW	1	Fair.
23	50	7 0	56	66	30,02	75		NE	2	Fair.
	62	2 0	60	66	30,08	65		NE	2	Fair.
24	46	7 0	52	64	30,14	75		N	1	Fine.
	68	2 0	66	65	30,09	66		NW	1	Fine.
25	55	7 0	58	64	30,00	80		W	1	Cloudy.
	68	2 0	67	65	30,00	72		W	1	Cloudy.
26	55	7 0	57	64	29,86	80		S	1	Cloudy.
	61	2 0	60	64	29,79	86		S	1	Rain.
27	56	7 0	56	64	29,70	94	0,340	NE	1	Rain.
	62	2 0	59	64	29,85	87		NE	1	Cloudy.
28	48	7 0	52	63	30,10	81	0,060	NE	1	Fine.
	64	2 0	63	63	30,12	68		NE	1	Fair.
29	50	7 0	52	62	30,22	80		E	1	Fine.
	72	2 0	70	64	30,18	67		S	2	Fair.
30	58	7 0	60	63	30,03	87		SSW	2	Cloudy.
	65	2 0	65	64	29,91	92		SW	1	Rain.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
July 1	53	7	0	57	64	29,97	80	0,063	W	2	Cloudy.
	66	2	0	66	64	30,05	73		WNW	1	Cloudy.
2	53	7	0	56	63	30,17	82		N	1	Cloudy.
	69	2	0	68	64	30,21	72		N	1	Cloudy.
3	61	7	0	63	64	30,11	80		W	1	Cloudy.
	74	2	0	73	65	30,04	72		WNW	1	Cloudy.
4	61	7	0	61	64	29,92	88		E	1	Rain.
	68	2	0	66	65	29,92	81		E	1	Cloudy.
5	53	7	0	57	64	29,95	84	0,030	E	1	Fine.
	72	2	0	71	66	29,92	72		E	1	Fair.
6	59	7	0	60	64	29,85	85		NE	1	Cloudy.
	62	2	0	61	65	29,85	84		NNE	1	Cloudy.
7	54	7	0	55	64	29,68	82	0,087	WSW	1	Cloudy.
	67	2	0	63	64	29,55	84		S	2	Cloudy.
8	54	7	0	56	64	29,64	85	0,095	SW	1	Fine.
	72	2	0	69	65	29,66	78		S	2	Cloudy.
9	62	7	0	63	65	29,83	91		S	1	Cloudy.
	75	2	0	73	66	29,96	78		S	1	Cloudy.
10	58	7	0	61	66	30,15	87		SW	1	Hazy.
	81	2	0	81	68	30,13	72		S	1	Fair.
11	67	7	0	72	68	29,97	81		E	2	Fair.
	73	2	0	68	68	29,92	85		SW	1	Cloudy. [this forenoon much lightning & thunder.
12	57	7	0	59	69	30,03	84	0,643	SW	1	Fine.
	74	2	0	74	69	30,05	67		WNW	1	Fair.
13	62	7	0	62	68	29,91	83		SW	2	Cloudy.
	77	2	0	76	69	29,90	66		SW	2	Fair.
14	64	7	0	65	69	29,74	85	0,102	SSW	2	Cloudy.
	71	2	0	67	69	29,68	85		SW	2	Rain.
15	55	7	0	57	67	29,85	87	0,615	SW	1	Fine.
	72	2	0	70	68	29,82	73		S	2	Fair.
16	56	7	0	57	67	29,65	83		SW	2	Fine.
	70	2	0	69	68	29,65	73		WSW	2	Cloudy.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°					Inches.	°	Inches.			
July 17	53	7	0	57	66	29,70	85		SW	2	Fine.
	68	2	0	68	67	29,68	78		SW	2	Rain. [Lightning, with thunder
18	53	7	0	57	66	29,75	84	0,210	S	2	Cloudy.
	67	2	0	62	66	29,76	82		SW	1	Rain.
19	54	7	0	56	65	29,78	86	0,392	NW	1	Cloudy.
	67	2	0	65	66	29,86	77		N	1	Fair.
20	53	7	0	56	65	29,96	83	0,040	WSW	1	Cloudy.
	70	2	0	67	64	29,95	71		W	1	Cloudy.
21	56	7	0	57	65	29,85	84		SW	1	Fair.
	70	2	0	66	65	29,81	74		SSW	1	Cloudy.
22	52	7	0	55	65	29,82	87	0,092	SW	1	Foggy.
	71	2	0	71	65	29,78	72		E	1	Fair.
23	57	7	0	57	64	29,54	87	0,020	E	1	Rain.
	69	2	0	68	65	29,51	90		E	1	Rain.
24	58	7	0	61	65	29,60	100	0,565	NE	1	Cloudy.
	68	2	0	63	65	29,62	87		SE	1	Rain. [much lightning, with thunder & heavy rain.
25	58	7	0	60	64	29,61	100	1,433	NE	1	Cloudy.
	68	2	0	67	65	29,63	78		NNE	1	Cloudy.
26	55	7	0	56	65	29,60	86		W	1	Fine.
	72	2	0	70	66	29,59	70		W	1	Fair.
27	55	7	0	58	65	29,67	88		SW	1	Cloudy.
	72	2	0	70	66	29,63	71		SSW	1	Cloudy.
28	61	7	0	59	65	29,59	91		ESE	1	Hazy.
	71	2	0	70	66	29,59	78		SE	1	Cloudy.
29	58	7	0	59	66	29,65	90		E	1	Fine.
	73	2	0	73	67	29,67	75		NE	1	Fine.
30	59	7	0	61	66	29,83	85	0,250	NE	1	Cloudy.
	75	2	0	69	67	29,83	80		SW	2	Rain.
31	60	7	0	61	66	29,73	91	0,252	S	2	Cloudy.
	74	2	0	73	68	29,71	70		SW	2	Fair.

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		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°					°				
Aug. 1	57	7 0	60	67	29,80	87		SSW	2	Cloudy.
	69	2 0	66	67	29,69	80		S	2	Cloudy.
2	57	7 0	58	66	29,55	85	0,042	SW	2	Fine.
	70	2 0	69	67	29,54	73		WSW	2	Fair.
3	56	7 0	58	66	29,78	83	0,200	WSW	1	Cloudy.
	71	2 0	70	67	29,82	70		WSW	1	Fair.
4	54	7 0	58	66	29,95	83		W	1	Cloudy.
	71	2 0	68	67	30,03	73		NNE	2	Fair.
5	55	7 0	57	66	30,14	80		WSW	1	Fair.
	72	2 0	71	67	30,06	68		WSW	1	Cloudy.
6	56	7 0	57	66	29,98	88		W	1	Cloudy.
	72	2 0	70	67	30,00	68		W	1	Cloudy.
7	60	7 0	62	66	29,98	90		WSW	1	Cloudy.
	76	2 0	75	67	29,97	74		W	1	Cloudy.
8	60	7 0	62	67	29,96	86		SW	1	Fine.
	77	2 0	76	68	29,94	72		SSE	1	Cloudy.
9	61	7 0	64	67	29,88	87		E	1	Fine.
	80	2 0	79	70	29,84	71		ESE	1	Fair.
10	58	7 0	59	67	29,94	83		WSW	1	Fine.
	76	2 0	75	70	29,97	70		NW	1	Fine.
11	58	7 0	61	69	30,00	77		NE	1	Fair.
	75	2 0	74	70	29,98	70		NE	1	Cloudy.
12	61	7 0	62	68	29,94	82		SW	1	Fair.
	76	2 0	74	70	29,90	70		WSW	1	Fair.
13	55	7 0	57	68	29,81	86	0,300	SW	1	Rain.
	68	2 0	67	68	29,68	83		SW	1	Rain.
14	54	7 0	56	67	29,78	83	0,660	WNW	1	Fair.
	70	2 0	67	68	29,77	71		WNW	1	Fair.
15	53	7 0	55	66	29,91	82	0,175	WSW	1	Fine.
	69	2 0	68	67	29,98	72		WSW	1	Fair.
16	55	7 0	57	66	30,17	84		SW	1	Fine.
	72	2 0	71	67	30,18	70		WNW	1	Fair.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°						°				
Aug. 17	58	7	0	60	67	30,20	80		SW	1	Cloudy.
	76	2	0	75	67	30,17	68		SSW	1	Fair.
18	57	7	0	60	67	30,12	82		SSW	1	Fine.
	75	2	0	75	69	30,09	70		E	1	Fine.
19	57	7	0	62	68	30,00	87		NE	1	Cloudy.
	73	2	0	73	69	29,92	74		E	1	Fair.
20	62	7	0	64	68	29,78	91	0,035	NE	1	Cloudy.
	73	2	0	72	69	29,74	85		S	1	Rain.
21	61	7	0	63	68	29,67	88	0,280	S	2	Cloudy.
	71	2	0	68	69	29,66	78		SE	2	Cloudy.
22	61	7	0	59	68	29,75	87		SW	2	Fine.
	76	2	0	76	69	29,83	70		SW	2	Fine.
23	60	7	0	63	69	29,93	86		SW	2	Cloudy.
	74	2	0	73	70	29,93	72		S	2	Fine.
24	57	7	0	58	69	30,07	84		SW	1	Fine.
	72	2	0	70	69	30,09	70		W	1	Fair.
25	53	7	0	55	68	30,18	85		SW	1	Fine.
	71	2	0	71	69	30,06	69		S	1	Fair.
26	60	7	0	62	67	29,64	81		S	2	Cloudy.
	63	2	0	63	67	29,44	86		S	2	Rain.
27	55	7	0	55	66	29,37	86	0,160	S	2	Rain.
	68	2	0	67	67	29,56	73		SW	2	Fair.
28	58	7	0	56	66	29,68	83	0,055	S	2	Cloudy.
	68	2	0	67	66	29,57	73		SSE	2	Cloudy.
29	59	7	0	62	65	29,28	84		E	1	Cloudy.
	67	2	0	65	66	29,25	84		E	1	Rain.
30	53	7	0	56	62	29,63	84	0,180	SW	1	Cloudy.
	66	2	0	66	65	29,68	78		S	1	Fair.
31	51	7	0	54	64	29,77	86		E	1	Fine.
	71	2	0	70	65	29,80	72		S	1	Fine.

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1806	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
Sep. 1	54	7 0	55	64	29,84	87		SSW	1	Fair.
	72	2 0	71	66	29,91	73		WSW	1	Fair.
2	57	7 0	59	64	30,06	88		WSW	1	Cloudy.
	71	2 0	70	66	30,11	73		WNW	1	Cloudy.
3	59	7 0	61	65	30,06	85		SE	1	Cloudy.
	71	2 0	70	66	29,98	72		SSW	1	Fair.
4	60	7 0	60	65	29,82	90		S	1	Cloudy.
	68	2 0	68	66	29,71	78		S	1	Cloudy.
5	52	7 0	52	64	29,82	86	0,245	SW	1	Fine. [much lightning last night.
	68	2 0	68	67	29,97	72		W	1	Fair.
6	53	7 0	56	64	30,05	84		NE	1	Hazy.
	71	2 0	71	67	30,00	71		E	1	Fine.
7	59	7 0	61	66	29,93	86		SE	1	Cloudy.
	73	2 0	73	68	29,95	76		S	2	Fair.
8	58	7 0	58	67	29,95	87		S	1	Fine.
	71	2 0	71	68	30,00	70		SW	1	Fine.
9	56	7 0	56	67	29,83	86		NE	1	Rain.
	57	2 0	55	65	29,53	98		SW	1	Rain.
10	50	7 0	52	64	29,74	89	0,950	WSW	1	Fair.
	66	2 0	65	65	29,76	75		SW	1	Fair.
11	51	7 0	52	63	29,64	87	0,120	W	1	Cloudy.
	62	2 0	62	64	29,71	73		NW	1	Fair.
12	48	7 0	49	62	29,91	83		WNW	1	Fine.
	62	2 0	61	62	30,00	76		NW	1	Fair.
13	50	7 0	51	61	30,12	77		N	1	Cloudy.
	62	2 0	61	62	30,14	76		N	1	Fair.
14	49	7 0	51	61	30,04	92		NE	1	Cloudy.
	65	2 0	64	62	29,91	82		NE	1	Fair.
15	54	7 0	54	61	29,84	88		NE	2	Cloudy.
	58	2 0	56	60	29,95	81		NNE	1	Cloudy.
16	46	7 0	48	59	30,12	84	0,130	NE	2	Fine.
	61	2 0	61	61	30,17	70		N	1	Fine.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
Sep. 17	50	7 0	50	59	30,25	86		NW	1	Fair.
	65	2 0	63	60	30,27	71		N	1	Cloudy.
18	49	7 0	51	59	30,27	82		W	1	Cloudy.
	66	2 0	66	61	30,24	77		SW	1	Fair.
19	58	7 0	59	60	30,26	95		SSW	1	Rain.
	68	2 0	68	62	30,24	80		SW	1	Fair.
20	59	7 0	60	62	30,21	90		SW	1	Cloudy.
	70	2 0	69	63	30,20	77		WSW	1	Fair.
21	56	7 0	57	62	30,20	91		S	2	Cloudy.
	69	2 0	69	63	30,16	78		SW	1	Fair.
22	50	7 0	51	62	30,02	90		SW	1	Fair.
	71	2 0	71	65	29,96	78		S	1	Fine.
23	55	7 0	55	63	29,96	91		SW	1	Cloudy.
	66	2 0	66	64	29,97	81		S	2	Cloudy.
24	53	7 0	53	63	30,05	90		NNE	2	Cloudy.
	61	2 0	59	62	30,11	80		NE	1	Cloudy.
25	46	7 0	48	61	30,24	85		NE	1	Fine.
	61	2 0	61	62	30,26	78		NE	1	Fine.
26	44	7 0	44	60	30,27	87		SW	1	Fine.
	65	2 0	64	62	30,22	74		SW	1	Fine.
27	53	7 0	55	60	30,19	95		SW	1	Cloudy.
	67	2 0	66	62	30,17	80		S	1	Fine.
28	51	7 0	52	61	30,12	90		E	1	Cloudy.
	64	2 0	63	62	30,07	82		E	1	Cloudy.
29	58	7 0	58	61	29,86	96		NE	1	Cloudy.
	68	2 0	64	62	29,80	95		E	1	Rain.
30	51	7 0	51	61	29,90	93	0,475	SW	1	Fair.
	63	2 0	63	63	29,94	78		SSW	1	Fair.

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1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Oct. 1	47	7	0	47	59	30,02	90		S	1	Foggy.
	60	2	0	60	60	30,02	77		S	1	Fair.
2	47	7	0	49	59	29,98	91		SW	1	Fair.
	62	2	0	62	61	29,93	81		S	1	Fair.
3	56	7	0	58	60	29,70	97	0,070	S	1	Rain.
	63	2	0	60	61	29,57	90		S	2	Rain.
4	54	7	0	54	60	29,61	95	0,100	E	1	Cloudy.
	63	2	0	63	61	29,75	83		W	1	Cloudy.
5	52	7	0	52	60	30,00	88		W	1	Fair.
	64	2	0	63	61	30,03	80		WSW	1	Fair.
6	57	7	0	57	60	30,09	88		WSW	1	Cloudy.
	65	2	0	64	62	30,16	73		W	1	Fair.
7	48	7	0	49	60	30,33	87		SSW	1	Cloudy.
	59	2	0	59	60	30,33	82		WNW	1	Cloudy.
8	56	7	0	56	60	30,29	90		SW	1	Cloudy.
	61	2	0	60	61	30,27	70		WNW	1	Cloudy.
9	54	7	0	55	60	30,23	80		NE	1	Cloudy.
	63	2	0	63	61	30,18	74		E	1	Cloudy.
10	52	7	0	54	60	30,10	92		NE	1	Cloudy.
	60	2	0	60	61	30,07	78		E	1	Cloudy.
11	53	7	0	54	60	30,02	82		ENE	1	Cloudy.
	60	2	0	59	62	30,02	77		E	1	Fine.
12	48	7	0	49	60	30,02	86		E	1	Fine.
	59	2	0	58	62	30,01	76		E	1	Fine.
13	45	7	0	46	60	29,92	88		NE	1	Fine.
	59	2	0	59	62	29,84	80		E	1	Fine.
14	53	7	0	55	60	29,67	95		E	1	Cloudy.
	62	2	0	62	62	29,69	84		E	1	Cloudy.
15	50	7	0	50	60	29,74	95	0,088	SW	1	Cloudy.
	60	2	0	60	62	29,75	84		SSW	1	Fine.
16	52	7	0	53	60	29,77	92		ESE	1	Cloudy.
	56	2	0	56	60	29,74	84		ESE	1	Cloudy.

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	°			°	°	Inches.	°	Inches.			
Oct. 17	46	7	0	47	58	29,83	82		E	1	Fine.
	54	2	0	54	60	29,80	76		E.	1	Fine.
18	43	7	0	43	58	29,58	87		NE	1	Fair.
	54	2	0	53	59	29,52	76		NE	1	Fair.
19	44	7	0	48	55	29,51	87		NE	2	Fair.
	57	2	0	55	58	29,57	78		NE	2	Fair.
20	42	7	0	43	57	29,67	86		NE	1	Cloudy.
	53	2	0	53	58	29,60	81		E	1	Cloudy.
21	45	7	0	51	57	29,32	87		SE	2	Cloudy.
	56	2	0	55	58	29,05	92		S	1	Rain.
22	47	7	0	51	57	28,98	93	0,220	S	1	Cloudy.
	51	2	0	51	58	28,66	95		NE	1	Rain.
23	39	7	0	40	53	29,59	84	0,315	WNW	2	Fine.
	46	2	0	46	57	29,87	74		WNW	2	Fine.
24	35	7	0	38	54	30,05	82		W	1	Cloudy.
	49	2	0	49	57	30,17	75		W	1	Fine.
25	42	7	0	43	54	30,27	82		S	1	Cloudy.
	51	2	0	51	58	30,15	78		S	2	Cloudy.
26	50	7	0	52	55	30,00	87		S	2	Fine.
	64	2	0	64	61	30,01	80		S	1	Fine.
27	51	7	0	52	57	30,00	93		S	1	Fair.
	62	2	0	61	60	29,94	80		S	1	Fair.
28	51	7	0	53	58	29,79	92		SSE	1	Fair.
	62	2	0	62	60	29,85	87		S	1	Cloudy.
29	47	7	0	47	58	30,18	92		SW	1	Fair.
	56	2	0	55	59	30,23	91		WSW	1	Fair.
30	46	7	0	47	58	30,18	94		SE	1	Foggy.
	55	2	0	55	60	30,11	88		E	1	Fine.
31	47	7	0	49	58	29,90	92		E	1	Cloudy.
	53	2	0	53	58	29,82	88		E	1	Cloudy.

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for November, 1806.

1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Nov. 1	51	7	0	52	58	29,70	93	0,030	E	1	Rain.
	60	2	0	60	59	29,60	91		S	1	Fair.
2	51	7	0	53	59	29,34	90		SSE	2	Fair.
	56	2	0	56	59	29,20	90		S	2	Rain.
3	49	7	0	50	58	29,07	90	0,130	S	2	Rain.
	55	2	0	55	59	29,06	84		S	2	Cloudy.
4	50	7	0	50	58	29,14	94	0,300	NE	1	Rain.
	53	2	0	52	59	29,18	92		N	1	Cloudy.
5	48	7	0	48	58	29,40	93	0,445	W	1	Cloudy.
	53	2	0	52	59	29,48	80		W	1	Fair.
6	38	7	0	39	57	29,66	88		W	1	Fair.
	49	2	0	49	59	29,83	80		NW	1	Fine.
7	36	7	0	36	56	30,17	87		W	1	Fair.
	50	2	0	50	59	30,24	84		W	1	Fine.
8	45	7	0	47	57	30,30	90		S	1	Fair.
	56	2	0	56	57	30,30	82		S	1	Cloudy.
9	46	7	0	46	56	30,28	91		SSE	1	Fair.
	53	2	0	53	57	30,22	80		S	1	Cloudy.
10	41	7	0	42	55	30,07	88		SE	1	Cloudy.
	50	2	0	49	55	30,10	88		E	1	Foggy.
11	47	7	0	48	55	30,21	93		E	1	Cloudy.
	51	2	0	51	57	30,18	85		E	1	Cloudy.
12	38	7	0	38	55	30,07	90		E	1	Foggy.
	47	2	0	46	55	30,04	91		W	1	Cloudy.
13	44	7	0	46	54	30,06	93		SW	1	Cloudy.
	56	2	0	54	56	30,17	87		SW	1	Cloudy.
14	47	7	0	51	56	30,23	92		SW	1	Cloudy.
	53	2	0	53	58	30,15	93		SW	1	Cloudy.
15	51	7	0	51	56	29,93	93		SW	1	Cloudy.
	54	2	0	54	57	29,85	93		SW	1	Cloudy.
16	43	7	0	43	56	29,78	90	0,020	SW	1	Fair.
	51	2	0	51	57	29,75	82		SW	1	Cloudy.

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for November, 1806.

1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Nov. 17	43	7	0	46	56	29,94	90		WSW	1	Cloudy.
	54	2	0	53	57	30,00	84		SW	1	Cloudy.
18	48	7	0	52	56	29,88	93		S	2	Rain.
	56	2	0	56	57	29,80	91		S	2	Cloudy.
19	51	7	0	51	56	29,62	90	0,042	SW	2	Cloudy.
	56	2	0	52	57	29,56	86		SW	2	Cloudy.
20	41	7	0	41	54	29,35	85	0,560	WSW	2	Fine.
	49	2	0	48	56	29,44	78		W	2	Fine.
21	42	7	0	47	54	29,13	87		SSW	2	Rain.
	48	2	0	47	56	29,05	81		SW	2	Cloudy.
22	37	7	0	37	53	29,35	88		SW	1	Fine.
	47	2	0	47	57	29,53	80		W	2	Fine.
23	36	7	0	39	53	29,72	88		SW	1	Cloudy.
	53	2	0	49	54	29,51	93		S	2	Rain.
24	47	7	0	50	53	29,85	95	0,093	SW	1	Cloudy.
	56	2	0	56	56	29,92	95		SW	1	Cloudy.
25	43	7	0	46	53	29,71	95		SSW	2	Cloudy.
	58	2	0	56	57	29,71	93		SW	1	Cloudy.
26	51	7	0	51	54	29,38	90	0,280	SW	2	Rain.
	52	2	0	52	57	29,38	91		SW	2	Rain.
27	46	7	0	46	56	29,90	90	0,286	WNW	1	Fair.
	50	2	0	50	57	30,07	87		WNW	1	Cloudy.
28	49	7	0	56	57	30,00	97	0,305	S	1	Rain.
	59	2	0	58	58	30,00	95		S	2	Cloudy.
29	56	7	0	56	57	29,77	95	0,040	SW	2	Cloudy.
	59	2	0	58	59	29,60	86		SW	2	Cloudy.
30	41	7	0	42	55	29,80	87	0,020	SW	2	Fine.
	48	2	0	47	57	29,79	83		WNW	2	Fair.

METEOROLOGICAL JOURNAL
for December, 1806.

1806	Six's Therm. least and greatest Heat.	Time. H.	Time. M	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Dec. 1	42	8	0	46	55	29,15	90	0,070	SW	2	Rain.
	52	2	0	52	57	29,05	83		SW	1	Cloudy.
2	42	8	0	42	54	28,72	90	0,555	WSW	2	Rain.
	44	2	0	43	56	28,82	79		WNW	2	Cloudy.
3	40	8	0	42	54	29,58	88		WNW	1	Cloudy.
	46	2	0	46	56	29,70	83		NW	1	Cloudy.
4	41	8	0	41	53	29,75	91		N	1	Foggy.
	47	2	0	46	56	29,75	81		NW	1	Cloudy.
5	46	8	0	51	54	29,52	97		SW	2	Cloudy.
	54	2	0	54	56	29,46	83		SW	2	Cloudy.
6	52	8	0	53	55	29,33	90		S	2	Cloudy.
	54	2	0	51	57	29,37	83		SSW	2	Cloudy.
7	40	8	0	40	54	29,33	85		S	2	Fine.
	49	2	0	49	55	29,20	90		S	2	Rain.
8	44	8	0	44	53	29,07	88	0,105	S	1	Cloudy.
	46	2	0	46	54	29,12	86		E	1	Fair.
9	42	8	0	44	53	29,37	88	0,053	WNW	1	Cloudy.
	47	2	0	47	55	29,46	87		NW	1	Cloudy.
10	40	8	0	42	52	29,38	90	0,058	W	1	Cloudy.
	45	2	0	44	53	29,38	83		NW	1	Cloudy.
11	43	8	0	45	52	29,37	91		WSW	1	Cloudy.
	51	2	0	49	55	29,38	86		SSW	1	Cloudy.
12	43	8	0	43	53	29,32	85		SW	1	Fine.
	50	2	0	50	56	29,43	82		SSW	1	Fair.
13	48	8	0	56	53	29,10	92	0,145	S	2	Rain.
	57	2	0	54	57	29,08	90		S	2	Rain.
14	48	8	0	48	54	29,45	85	0,255	S	1	Cloudy.
	50	2	0	48	56	29,40	90		S	1	Rain.
15	45	8	0	45	53	29,63	91	0,610	SSW	1	Rain.
	47	2	0	46	56	29,75	85		SW	1	Fine.
16	42	8	0	53	54	29,67	93	0,095	SW	2	Rain.
	56	2	0	56	57	29,65	90		SSW	2	Cloudy.

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for December, 1806.

1806	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°						°				
Dec. 17	54	8	0	54	55	29,67	92	0,020	SSW	2	Cloudy.
	56	2	0	56	58	29,68	90		SSW	2	Cloudy.
18	50	8	0	50	56	29,70	91	0,130	SSW	1	Rain.
	52	2	0	50	58	29,75	88		S	1	Rain.
19	48	8	0	49	57	29,61	88	0,090	SE	1	Rain.
	53	2	0	51	58	29,62	85		W	1	Fine.
20	45	8	0	49	56	29,54	88	0,020	SE	2	Fine.
	53	2	0	52	58	29,34	84		S	2	Fair.
21	40	8	0	42	56	29,38	85	0,140	ESE	1	Cloudy.
	47	2	0	45	58	29,32	88		NE	2	Rain.
22	39	8	0	50	55	29,52	92	0,270	S	2	Rain.
	57	2	0	56	57	29,54	88		S	2	Cloudy.
23	52	8	0	55	57	29,87	95		SW	1	Cloudy.
	57	2	0	56	58	29,96	93		SSW	1	Rain.
24	55	8	0	57	57	30,02	90		SSW	2	Cloudy.
	57	2	0	51	59	30,20	85		N	2	Fair.
25	43	8	0	53	56	30,23	97		SSW	2	Rain.
	55	2	0	55	59	30,10	88		SSW	2	Cloudy.
26	42	8	0	43	56	30,24	90	0,025	WSW	1	Fine.
	47	2	0	47	58	30,28	82		WSW	1	Fine.
27	42	8	0	45	55	30,20	92		SW	1	Fair.
	52	2	0	52	57	30,13	88		SW	1	Cloudy.
28	47	8	0	49	56	29,93	92		SW	1	Cloudy.
	53	2	0	53	58	29,84	85		SSW	1	Fine.
29	48	8	0	49	56	29,60	88		SSE	2	Cloudy.
	51	2	0	51	56	29,47	90		S	2	Rain.
30	43	8	0	47	55	29,57	92	0,016	SSW	1	Rain.
	54	2	0	54	56	29,59	90		SSW	1	Cloudy.
31	43	8	0	43	55	30,05	85	0,060	NNE	1	Cloudy.
	44	2	0	43	55	30,20	83		NE	1	Cloudy.

1806.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	55	28	42,5	55	29	42,7	58	48	53,1	30,13	28,65	29,52	96	78	88,3	1,837
February	56	30	43,4	56	32	43,8	57	46	52,4	30,28	29,33	29,84	96	72	87,6	0,535
March	56	26	42,7	56	26	43,0	58	47	53,6	30,45	28,90	29,72	95	74	87,0	1,328
April	64	31	45,7	64	33	46,1	59	50	54,9	30,47	29,38	30,02	94	71	84,0	0,245
May	75	43	57,8	73	46	58,8	67	56	61,9	30,43	29,40	29,89	97	65	80,7	1,020
June	83	46	62,5	83	52	63,5	71	62	65,9	30,53	29,60	30,10	94	53	75,9	0,505
July	81	52	64,5	81	53	64,0	69	63	65,8	30,21	29,51	29,81	100	66	81,3	4,889
August	80	51	64,5	79	54	64,9	70	62	67,3	30,20	29,25	29,85	91	68	79,0	2,087
September	73	44	59,5	73	44	59,6	68	59	63,0	30,27	29,53	30,02	98	71	82,7	1,920
October	65	35	53,2	64	38	53,7	62	53	59,1	30,33	28,66	29,87	97	70	84,9	0,793
November	59	36	49,1	56	37	49,7	59	53	56,5	30,30	29,05	29,76	97	78	88,8	2,551
December	57	39	48,0	56	40	48,8	59	52	55,8	30,28	28,72	29,58	97	79	88,3	2,717
Whole year			52,8			53,2			59,1			29,83			84,0	20,427

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

Variation of the Magnetic Needle.

1806.

June, 24° 8′, 6.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCVII.

PART II.

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PHILOSOPHICAL TRANSACTIONS.

VII. *On Fairy-rings.* *By* W. H. Wollaston, *M. D. Sec. R. S.*

Read March 12, 1807.

THE circles of dark-green grass frequently observed in old pastures, and known to most persons by the name of Fairy-rings, although in themselves of no importance, yet seem to claim some attention, if we consider the many ingenious attempts that have been made to explain their origin. On such a subject I shall be excused offering any examination of opinions previously formed by others, and shall therefore proceed briefly to relate such observations as I made, during a few years residence in the country, on the progressive changes of these circles, and which seem to me to lead to a clear and satisfactory conclusion.

That which first attracted my notice, was the position of certain fungi which are always to be found growing upon these circles, if examined in a proper season. In the case of mushrooms, I found them to be solely at the exterior margin of the dark ring of grass. The breadth of the ring

in that instance, measured from them toward the centre, was about twelve or fourteen inches, while the mushrooms themselves covered an exterior ring about four or five inches broad.

The position of these mushrooms led me to conjecture that progressive increase, from a central point, was the probable mode of formation of the ring. I was the more inclined to this hypothesis, when I found that a second species of fungus presented a similar arrangement, with respect to the relative position of the ring and fungi; for I observed, that in all instances the present appearance of fungi was upon the exterior border of a dark ring of grass. I thought it not improbable that the soil, which had once contributed to the support of fungi, might be so exhausted of some peculiar *pabulum* necessary for their production, as to be rendered incapable of producing a second crop of that singular class of vegetables. The second year's crop would consequently appear in a small ring surrounding the original centre of vegetation, and at every succeeding year the defect of nutriment on one side would necessarily cause the new roots to extend themselves solely in the opposite direction, and would occasion the circle of fungi continually to proceed by annual enlargement from the centre outwards. An appearance of luxuriance of the grass would follow as a natural consequence, as the soil of an interior circle would always be enriched by the decayed roots of fungi of the preceding years growth.

By reference to Dr. HUTTON'S* "Observations on certain natural appearances of the ground of the hill of Arthur's Seat near Edinburgh," we find the progressive enlargement

* Edinburgh Transactions.

distinctly noticed; but as he happened not to observe any of the fungi that occasioned them, he speaks of it merely as "a piece of natural history worth recording, and for which, a theory is wanting."

Respecting the enlargement, he says, "from all the observations I have made, this progress seems always to have proceeded in the direction of a line bisecting the segment, that is to say, those portions of concentric circles are never inscribed but always circumscribed; and for this reason it appears, that those circles of which segments are exhibited to our observation must be increasing and not diminishing in their diameters."

Although Dr. HUTTON has overlooked the real origin of these appearances, Dr. WITHERING has ascribed them to their true cause; but his remarks are confined to one species of agaric (the *Ag. orcades* of his Arrangement), and do not appear to have been confirmed by any subsequent observation of their annual progress.

"I am satisfied," says he, "that the bare and brown, or highly cloathed and verdant circles in pasture fields called Fairy-rings are caused by the growth of this agaric."—"Where the ring is brown and almost bare, by digging up the soil to the depth of about two inches, the spawn of the fungus will be found of a greyish white colour; but where the grass has again grown green and rank I have never found any of the spawn existing."

Had Dr. WITHERING frequently repeated this examination of the soil he would have corrected the last remark, which is not universally true, as the grass may at some period be found luxuriant even over the undecayed spawn. During the growth

of the fungi, they so entirely absorb all nutriment from the soil beneath, that the herbage is for a while destroyed, and a ring appears bare of grass surrounding the dark ring. If a transverse section be made of the soil beneath the ring at this time, the part beneath the fungi appears paler than the soil on either side of it, but that which is beneath the interior circle of dark grass is found on the contrary, to be considerably darker than the general surrounding soil. But in the course of a few weeks after the fungi have ceased to appear, the soil where they stood grows darker, and the grass soon vegetates again with peculiar vigour; so that I have seen the surface covered with dark grass, although the darkened soil has not exceeded half an inch in thickness, while that beneath has continued white with spawn for about two inches in depth.

The section of the space occupied by the white spawn has in general nearly the same form, and may be compared to that of a wave proceeding from the centre outwards, as its boundary on the inner side ascends obliquely toward the surface, while its exterior termination is nearly in a vertical position. The extent occupied by the spawn varies considerably according to the season of the year, being greatest after the fungi have come to perfection, and is reduced to its smallest dimensions, and may in some cases not be discernible before the next year's crop begin to make their appearance.

For the purpose of observing the progress of various circles I marked them three or four years in succession, by incisions of different forms, by which I could distinguish clearly the successive annual increase, and I found it to vary in different circles from eight inches to as much as two feet. The broadest rings that I have seen were those of the common

mushroom (*Ag. campestris*); the narrowest are the most frequent, and are those of the champignon (*Ag. orcades* of Dr. WITHERING). The mushroom accordingly makes circles of largest diameter, but those of the champignon are most regular. There are, however, as many as three other fungi that exhibit the same mode of extension, and produce the same effect upon the herbage. These are the *Ag. terreus*, *Ag. procerus*, and the *Lycoperdon bovista*, the last of which is far more common than the two last mentioned agarics.

There is one circumstance that may frequently be observed respecting these circles, which can satisfactorily be accounted for, according to the preceding hypothesis of the cause of their increase, and may be considered as a confirmation of its truth. Whenever two adjacent circles are found to interfere, they not only do not cross each other, but both circles are invariably obliterated between the points of contact: at least in more than twenty cases, I have seen no one instance to the contrary. The exhaustion occasioned by each obstructs the progress of the other, and both are starved.

I think it also not unworthy of observation, that different species of fungi appear to require the same nutriment; for in a case of interference between one circle of puff-balls and another of mushrooms, they did not intersect; but I cannot say positively that I have seen more than one instance.

I once found that a tree had interrupted the regular progress of a circle; but this appeared to be only a temporary impediment, as the extension had proceeded at the usual rate, and by passing obliquely from each side into the soil beyond the tree, had given the ring the form of a kidney, so that

another year or two would probably reunite the two extremities into one curve surrounding the tree.

Being desirous of ascertaining in what length of time a soil might again recover the power of producing a fresh crop of fungi, I cut a groove, in one or two instances, along the diameter of a mushroom-ring, and inserted a quantity of spawn taken from its circumference, with the hope of seeing it vegetate for some distance near the center; but the experiment failed altogether: and as I shortly after quitted my residence in the country, I had no opportunity of repeating the experiment, and must leave it to be prosecuted by those who are more favourably circumstanced.

VIII. *Observations on the Structure of the Stomachs of different Animals, with a View to elucidate the Process of converting animal and vegetable Substances into Chyle.* *By* Everard Home, *Esq. F.R.S.*

Read April 30, 1807.

THE observations on the stomachs of the porpoise, and of ruminating animals contained in two former communications, led me to believe that the fourth cavity of the ruminant's stomach, while the animal is alive, is always divided, in a greater or less degree, into two portions, in one of which is included the plicated structure, in the other, the villous. In some genera, this division is permanent, as in the camel and that tribe; in others only occasional, as in the bullock, deer, sheep, &c.

If this opinion should be found to be true with respect to animals in general, it will throw considerable light on the processes carried on in the stomach, and lead us to conclude, that the food undergoes two changes in it, the one preparatory to the other, and that it is the last of these, which forms the chyle.

With a view to investigate still further this very interesting subject, I have been led to examine the internal structure of the stomachs of different animals.

In this enquiry it will be found that the same substances are digested by stomachs varying considerably from each other, and many of these varieties can at present in no other way be accounted for, than by referring them to the general principle, which pervades the structure of animals, making them run into one another by a regular series of minute changes of form, so as to compose one connected chain, from which we derive the fullest evidence of the power and wisdom of their Creator.

The stomachs of all ruminating animals have three different structures; the first of these is cuticular; the second has a secreting surface, thrown into folds, on which are seen the orifices of glands; and the third is smooth, and more delicate in its texture.

In the following account, it will be found that three similar structures are met with in the stomachs of quadrupeds which do not ruminate, and that the gradation between the most complex and most simple stomach forms an uniformly connected series, of greater extent than has been hitherto supposed.

To complete the view of this subject is too extensive a pursuit for an individual, whose professional duties occupy so large a portion of his time as mine necessarily do. All that can be expected from one so circumstanced is to give a general outline, leaving the minuter parts to be filled up by those who have more leisure, but by no means more zeal, for studies of this kind.

As these materials are collected not merely for the present investigation, but are intended for the use of future enquirers,

care has been taken that every anatomical fact brought forward, should be ascertained by actual examination.

It is proposed to describe the internal structure of such stomachs as appear to form the principal links in the gradation between animals which ruminate, and those which are truly carnivorous, arranging them in a series, the beginning of which is that nearest allied to the stomach of the ruminant, in the complexity of its parts, and the termination, that, which is most simple in its internal structure.

Before the appearances of these different stomachs are described, it will be necessary to explain the circumstances under which they were observed. As the object of the present enquiry is to determine with as much accuracy as possible the shape, the stomach puts on, while performing its functions in the living body, and the structure, which belongs to the different parts of its internal membrane, it became necessary to consider what would be the best mode of making such examinations. It was found that the stomach ought not to be in a distended state at the time of the animal's death, for when that is the case, the air which is let loose or even the shaking of the contents, elongates or stretches the muscular fibres, so as to enlarge the cavity, and give it a form, by no means natural to it. This partly arises from the weakness of the muscular fibres themselves; but principally from the effect of death upon this organ, which destroys the rigidity of its muscular fibres, so that they become easily elongated, even when much shortened at the time death takes place. It is necessary to mention this circumstance, as it is the reverse of what happens in the voluntary muscles, which are generally known to become rigid at that time, and it accounts for the

real form of the stomach having been much less frequently noticed, than was naturally to be expected.

To come at the real form of the stomach, it must be seen recently after death, before its muscles have been disturbed; in this state a gentle and gradual distension with air shews both the permanent divisions of its cavity, if there are any, in the best possible manner, and also any occasional muscular contractions, that are employed during life.

The internal membrane is only to be met with in a natural state recently after death, since the secretion from the solvent glands frequently acts upon it, and destroys the surface, and the slightest degree of putrefaction, which comes on very quickly in this cavity, prevents the nicer distinctions of structure from being detected.

To make an accurate examination of the different parts of this membrane, it is necessary that its folds should be extended, and the mucus commonly found adhering to it removed, which is most readily effected, and with the least disturbance, by inverting the stomach and gradually distending it; and in this state only can the relative situation of the different structures be ascertained with exactness.

In examining stomachs, with the attention directed to all the circumstances above mentioned, it is found that in a recent state, the internal membrane is often completely obscured by mucus, which in many instances is inspissated, and puts on the appearance of a cuticular covering, from which it is with difficulty distinguished; in others it resembles a fine villous surface, so very tenacious is its nature; and where the membrane is irregular it adheres with unusual firmness.

The internal membrane of most stomachs is found to be

considerably more extensive than any of the other coats, and much more so than it appears to be on a superficial examination; for it is not only thrown into longitudinal and transverse folds, but is subdivided by slight fissures into a number of small portions varying in shape and size in different parts of the same stomach, but generally smallest near the pylorus; this appearance was at first mistaken for the real internal structure of the membrane; but when inverted and distended, so as to be put upon the stretch all these disappeared, and it became very thin and smooth. This is seen most readily in the human stomach, and in those of carnivorous animals.

Such distension enables us to examine the internal structure of parts, but this is not to lead us away from their more natural appearance; since the functions of this membrane could no more go on were it unfolded to a great extent, than the muscular actions of the outer coat, in an over-stretched state of its fibres.

In proof of this observation, I have known an instance of a child three years old, who being left alone at dinner, ate so large a quantity of apple-pudding that it died, which raised suspicion of its having been poisoned. On examination after death, the whole stomach was distended to its utmost extent, and rendered quite tense, which was the only apparent cause of the child's death.

Having made these general remarks, which will render the following descriptions intelligible, without entering into detail on the mode of examining each particular stomach, I shall proceed to describe those stomachs, from which I mean to draw conclusions respecting digestion. The drawings, illustrate the appearances so well, that a short account

will be sufficient. They are all made by Mr. CLIFT, whose knowledge of anatomy has enabled him to delineate the different parts with a degree of correctness, which could not have been otherwise attained. As I so often avail myself of his talents, I am desirous of acknowledging on all occasions the benefit I derive from them. In procuring and examining the different stomachs, I have received material assistance from Mr. BRODIE.

No. I. *The Turkey.*

Immediately below the crop the œsophagus of the turkey is lined with a cuticle, and when narrowly observed a great many small orifices leading to glands belonging to that canal are very distinctly seen. This cuticular lining terminates in a line across the œsophagus, immediately above the solvent glands, or as they have been hitherto termed, the glands that secrete the gastric juice. The surface on which their ducts open is not cuticular, but membraneous; their orifices are placed in six rows across the canal. Each of them has a prominent or nipple-like appearance, and they are nearly at equal distances from each other. Immediately below the solvent glands is the entrance into the gizzard, composed of longitudinal ridges, covered with the same kind of horny cuticle which lines the sides of the gizzard itself.

At the lower orifice of the gizzard, just before the duodenum begins, there is a surface of about half an inch in extent, which has a delicate structure, composed of very minute parts; these are not distinct to the naked eye, but when magnified by a lens whose focus is $1\frac{1}{4}$ inch, they appear to be granules, separated by interstices from each other. This

portion I consider to belong to the stomach, and to form a part of the digestive organ. It terminates by a tolerably defined line, where the villous appearance of the duodenum begins. Vide Plate V. fig. 1.

No. II. *The Cod Fish.*

The stomach of the cod is a direct continuation of the œsophagus, from which its origin is only to be distinguished by the termination of the cuticular lining. It is made up of two cavities; one large, which I shall call the cardiac portion, the other small, which I shall call the pyloric. The cardiac cavity terminates in a rounded extremity; and on one side, a little above its termination, it communicates with the pyloric by a very narrow opening.

From the cuticular lining of the œsophagus project a number of small processes, as in the turtle, but formed on an infinitely smaller scale; these, when examined, do not appear to be tubular, any more than those in the turtle. The internal membrane of the cardiac portion is thrown into longitudinal folds of an irregular form, and there is a secretion of viscid mucus from every part of its surface; there are also numerous orifices distinctly seen on the prominent parts of these folds, which I consider to belong to the solvent glands. These are in greatest number towards the lowest extremity, but are met with towards the upper end.

The pyloric cavity has an internal surface of a very different kind; it resembles a fine honey-comb, or network. Vide Plate V. fig. 2.

No. III. *The Hare.*

The stomach of the hare, when forcibly distended, appears to be one nearly uniform cavity; but when examined immediately after death, before the parts have been disturbed, is found to have a partial contraction, dividing it into two; the cardiac portion is two-thirds and the pyloric portion is one-third of the whole cavity.

The muscular coat of the cardiac portion is weak, but at the division between it and the pyloric the fibres become much stronger: they are regularly circular, and continue so half way to the pylorus; there they form a thick projecting band, and afterwards become spiral, towards the pylorus. There are two layers of these spiral fibres in opposite directions, crossing each other, which gives them great power in their contraction, and very considerably increases its effect.

The internal membrane of the cardiac portion forms one uniform surface. Where the pyloric portion begins the membrane is thicker in its substance, and the surface more villous; further on, where it is surrounded by the projecting band, there are small distinct orifices, largest, and in greatest number on the lower curvature, but met with all round; these appear to be the excretory ducts of the glands, which secrete the solvent liquor. From this part to the pylorus, the surface is smoother, and has a more delicate texture.

The rabbit's stomach corresponds in every respect with that of the hare, only that the parts are on a smaller scale, and less conspicuous. The orifices of the ducts above described were

not detected in the rabbit till they had been seen in the hare, but then were readily distinguished. Vide Plate V. fig. 4 and 5.

No. IV. *The Beaver.*

The stomach of the beaver is divided by a muscular contraction into two portions; the cardiac, which is of an oval form, may be called the descending portion; the pyloric, which is much smaller, and bent upwards, may be called the ascending portion: the contraction between them is sufficiently strong to bear the force necessary to distend the stomach, without yielding to it. The cuticular lining of the œsophagus terminates at the orifice of the stomach. Just within that orifice, upon the upper or small curvature is a large oval glandular structure, subdivided into three prominent ridges, placed in the direction of the stomach, and projecting into its cavity, one in the middle line, and one on each side of it: in the middle ridge there are nine large openings through the internal membrane, capable of contracting so as to shut up the orifices, or of dilating so as to expose three inner orifices leading to the gland; each of these is continued into five or six processes, whose length is proportioned to the thickness of the glandular mass, extending nearly to its external surface. In each of the lateral ridges there are seven orifices. The internal membrane of the descending portion of the stomach, into which these excretory ducts open, is uniformly smooth in every other part of it; but the lining of the smaller ascending portion has a villous appearance, subdivided by slight fissures: this however is only to be seen when minutely examined. The part next the pylorus has a strong muscular covering

of some thickness, similar to what is met with in the hare and rabbit. Vide Plate VI. fig. 1, 2, and 3.

No. V. *The Dormouse.*

The stomach of the dormouse is divided into two portions by a muscular contraction, which is very distinct when examined immediately after death. At the orifice of the stomach there is a peculiarity shewn to me several years ago by Mr. MACARTNEY, which I had never seen till that time.

This peculiarity is a glandular substance, surrounding the œsophagus, immediately before it terminates in the stomach; the orifices of which open on the internal membrane of the œsophagus. Mr. MACARTNEY left me a drawing of the external appearance of the gland, when the stomach was in a distended state, in which the muscular contraction between the two portions was destroyed. He said nothing about the structure of the gland, and as it was a subject, which did not then engage my attention, I thought no more about it. In the course of the present enquiry it occurred to my recollection, and upon comparing this gland with that of the beaver (with which it corresponds very minutely in its internal structure) it becomes a fact of no small importance in forming a series of glandular structures belonging to the stomach: in making use of this fact, I have great pleasure in acknowledging the source, from whence my first knowledge of it was derived. This glandular structure viewed externally is like a mulberry, being made up of a number of small projections; the orifices in the membrane of the œsophagus admit of distension with air, and when expanded each orifice exposes three

small openings; these again lead to several processes, as has been described and delineated in the glandular structure of the beaver.

The first portion of the stomach forms about $\frac{2}{3}$ of the whole, while the second is only the remaining third; internally the membrane has no peculiar appearance, and is uniformly the same in both portions. The cuticular lining of the œsophagus terminates immediately above the glandular structure, which has just been described; so that the stomach of the dormouse is in all respects very similar to that of the beaver. Vide Plate VI. fig. 4 and 5.

No. VI. *The Water Rat.*

The stomach of the water rat is made up of two cavities with a narrow communication between them. The cavity into which the œsophagus opens is nearly two-thirds, and the other rather more then one-third of the whole. The stomach terminates at the pylorus by a very contracted orifice.

The first cavity has a cuticular lining continued from the œsophagus over the whole of its internal surface, terminating in a prominent serrated edge at the contracted part, except that on each side an oval portion of cuticle extends into the second cavity; this is seen through the other coats of the stomach. There are no apparent orifices in this cuticular lining leading to glands. The œsophagus opens into it obliquely, so that regurgitation can hardly take place.

The second cavity is lined with a membrane, which, at the lower part or greater curvature, is thicker than at any other; the surface is convoluted, and appears to secrete a thick viscid mucus; beyond this there is an irregular zone of orifices, which I consider to be the ducts of the solvent glands. From

this part to the pylorus the membrane is more smooth, and made up of minuter parts. Vide Plate VI. fig. 6.

No. VII. *The Common Rat.*

The stomach of the common rat has a general resemblance in its appearance to that of the water rat, but differs from it in having no permanent division between the two portions of which it is composed. When examined recently after death there is a contraction dividing it into two parts; but when distended this disappears, and the whole becomes one cavity; so that in this animal the division is only muscular, which in the water rat is permanent.

The first cavity bears a greater proportion to the whole than in the water rat; it is about $\frac{3}{4}$ instead of $\frac{2}{3}$. The first cavity is lined with a cuticle, which terminates in a line like a thread, formed by a doubling of the cuticular edge, but not projecting or serrated, as in the water rat. This line surrounds the stomach, but projects furthest on the lower part or great curvature, where it terminates in a point: there are also the two lateral cuticular processes as in the water rat, but less conspicuous from not being prominent, and much smaller.

The internal surface of the second cavity so entirely corresponds with that of the water rat, as to require no particular description; only the orifices of what I consider to be the solvent glands are less readily detected.

The stomach of the mouse is similar to that of the common rat in its general characters.

No. VIII. *The Horse.*

The stomach of the horse, as it is most commonly met with after death, appears to be an oval bag, the internal surface of

which, next the great end, is covered with a cuticle continued from the œsophagus, and extending further towards the pylorus on the small curvature than on the great one. The œsophagus enters obliquely; which prevents regurgitation from readily taking place. At the great curvature, immediately beyond the termination of the cuticle, which forms a prominent ridge, there is a glandular structure of some extent: this is insensibly lost in the more membranous portion, which extends to the pylorus, and appears to be a villous surface subdivided into small portions of unequal size, giving it a tesselated appearance.

When the horse's stomach is procured in an empty state, or nearly so, immediately after death, and is inverted and gradually distended, it is found to consist of two very distinct portions, there being a muscular contraction between the cuticular portion and the other.

No. IX. *The Ass.*

The stomach of the ass resembles that of the horse in all respects, and being of a more delicate structure its minuter parts are more easily distinguished. A number of orifices of glands immediately beyond the cuticular portion on the upper curvature are distinctly seen, which I was unable to distinguish in the horse; but there can be no doubt of their existence in that animal, although I was not so fortunate as to observe them. Vide Plate VII.

No. X. *The Kanguroo.*

The stomach of the kanguroo differs in many particulars from that of any other known animal, and bears a much greater resemblance to the human cœcum and colon than to any stomach.

The œsophagus enters the stomach very near its left extremity, which, unlike the corresponding part in other animals, is very small and bifid. From the entrance of the œsophagus the cavity extends towards the right side of the body; then passes upwards, makes a turn upon itself, crosses over to the left side before the œsophagus, and again crosses the abdomen towards the right, making a complete circle round the portion into which the œsophagus enters, and terminates by a contracted orifice at the pylorus.

Its cavity gradually enlarges from the left extremity through its whole course, till it approaches the pylorus; it then contracts and dilates again into a rounded cavity, with two lateral processes: beyond this is the pylorus, the orifice of which is extremely small. On the anterior and posterior side of the stomach there is a longitudinal band similar to those of the human colon, beginning faintly at the left termination, and extending as far as the enlargement near the pylorus: these bands being shorter than the coats of the stomach, the latter are consequently puckered, forming sacculi, as in the human colon.

When the cavity of the stomach is laid open, the cuticular lining of the œsophagus is found continued over the portion immediately below it, and extends to the termination of the smallest process at the left extremity, and nearly to the same distance in the opposite direction; the cuticular covering is very thin, and extremely smooth.

The lining of the larger process at the left extremity is thick and glandular, and in the living body probably receives no part of the food, but is to be considered as a glandular appendage.

On the right of the œsophagus the cuticle does not end by a transverse line, but terminates first upon the middle line of the great curvature, where a villous surface begins by a point, and gradually encreases in breadth till it extends all round the cavity: its origin therefore is in the form of an acute angle. This villous surface is continued over the remaining cavity as far as the longitudinal bands extend: and that half of it next the pylorus has three rows of clusters of glands: one row is situated along the great curvature, and consists of 15 in number; the other two rows are close to the two longitudinal bands, and consist only of nine. Besides these there are two larger clusters of an oblong form, situated transversely, where the longitudinal bands terminate. The internal surface of the rounded cavity next the pylorus has a different structure, putting on a tesselated appearance, formed by a corrugated state of the membrane. Immediately beyond the pylorus is a ring of a glandular structure surrounding the inner surface of the duodenum. Vide Plate VIII.

No. XI. *The Hog.*

The general form of the stomach is nearly that of the beaver; it is divided by a muscular contraction into two portions. The cardiac large and oval, its direction obliquely downwards; the pyloric small and conical, its direction upwards. There is a process continued from the cardiac extremity, turned back upon the upper part of the stomach, which terminates in a blunted end.

The cuticular lining of the œsophagus extends along the small curvature of the stomach in both directions, and terminates at the base of the process above-mentioned. This process is sometimes found contracted and quite empty, so that it does

not appear to form a part of the receptacle for food. The internal membrane of the cardiac portion of the stomach, as far as the opening of the œsophagus, is an uniform surface, but immediately beyond, on the lower side or great curvature, there is a thick glandular substance of an oval form, bounded laterally by two prominent ridges, one on each side, with a similar one in the middle line of the stomach: there are also three smaller passing transversely from the middle to the lateral ones. A small part of this glandular structure is situated in the pyloric portion, and where it terminates there is a row of large orifices leading to glands. These only extend round the upper part, and are not continued beyond the edge of the glandular structure. The pyloric portion has a smooth villous appearance. Vide Plate IX.

No. XII. *The Pecari.*

The stomach of the pecari differs from that of the common hog, in there being two processes at the cardiac extremity, and these having a more lateral direction; so that the stomach appears to be composed of three bags; one the general cavity, the others the two lateral processes; that which projects anteriorly is nearly double the size of the other. The cuticular lining of the œsophagus extends further on the sides of the general cavity of the stomach than in the hog.

XIII. *The Elephant.*

This stomach is longer and narrower than that of most other animals; the whole length is three feet three inches; and its diameter in the middle line, which is the widest part, is one foot two inches. The cuticular lining of the œsophagus terminates at the orifice of the stomach. The internal membrane of the stomach differs in appearance in different parts. At the cardia

which is very narrow, and pointed at its extremity, the lining is thick and glandular for eight inches in extent, and is thrown into transverse folds, of which five are broad and nine narrow; that nearest the orifice of the œsophagus is the broadest, and appears to act occasionally as a valve, so that the part beyond may be considered as an appendage, similar to the processes in the hog and pecari; the membrane of the cardiac portion is uniformly smooth, that of the pyloric is thicker and more vascular.

These observations, as well as the engraving, are made from a dried preparation. Vide Plate X.

XIV. *The Mole.*

The cavity of the stomach when distended is of a globular form, and I have had no opportunity of examining it recently after death so as to ascertain where the division takes place between the cardiac and pyloric portions. Its coats are very thin and transparent. The œsophagus opens into it midway between the two extremities.

The cuticular lining of the œsophagus terminates at the orifice of the stomach. The internal membrane is uniformly smooth except at the pylorus, where it is surrounded by a glandular zone immediately within the orifice, the surface of which has a granulated appearance.

XV. *The Stoat.*

The stomach when distended has a globular form; its coats are thin and transparent; I have not seen it in an undisturbed state; the œsophagus opens into it nearly midway between the two extremities.

The inner membrane has an uniformly smooth appearance, except near the pylorus, where it is thicker and has a glandular structure, the surface of which is granulated. This differs from that in the mole, by being spread over a surface of some extent, and the glands being deeper seated behind the internal membrane.

No. XVI. *The Armadillo (with nine Bands.)*

The stomach has an oval form; the œsophagus opens into it nearer to the pylorus than in many other animals.

The cuticular lining of the œsophagus terminates at the orifice of the stomach. The cavity of the stomach is divided into two portions by a muscular contraction. The surface of the cardiac portion is uniformly smooth, but in the pyloric portion, for the space of an inch and three quarters from the pylorus, the muscular coat is thicker and more compact, and immediately within the orifice of the pylorus there is a zone of a glandular structure surrounding it; the orifices leading to the glands are very apparent.

The animal, from which this description is taken, was given to me by Lord SEAFORTH. It died on the passage from the West Indies, and was immediately preserved in spirits; the parts did not admit of a drawing being made of them, but were in a more natural state respecting their contraction than in other animals where the stomach has been so long kept as to be distended with air.

No. XVII. *The Human Stomach.*

The human stomach, when examined recently after death, puts on appearances, that have not been noticed, which makes

the present description, and the drawing that accompanies it, necessary to explain these circumstances. It is occasionally divided by a muscular contraction into two portions; these are in shape, and relative size, sometimes similar to those of the beaver, at others to those of the horse. When its internal surface is accurately examined under the most favourable circumstances, the orifices of the œsophageal glands are distinctly seen in different parts, but more numerous just above where the cuticle terminates at the orifice of the cardia. Immediately within the cavity of the stomach, there are clusters of glands, exceedingly small and pellucid, crowded on one another, spread over the internal membrane of the small curvature for several inches in extent, but no where else. To have a distinct view of them requires the use of a magnifying glass; but when once observed, they are seen with the naked eye. The cardiac portion has an uniform surface, but towards the pylorus there is a more minute structure, very much resembling the appearance of a tesselated pavement, composed of very small portions of different forms. Vide Plate XI. fig. 1.

XVII. *The Lynx.*

The stomach was in a very flaccid state and quite empty. When inverted and gently distended with air its form resembles that of the beaver, only the pyloric portion is more bent upon the small arch.

The cuticular lining of the œsophagus terminates exactly at the orifice of the stomach by a serrated edge, and is thrown into a number of transverse folds. Immediately within the orifice of the stomach, and extending along the small arch, there are clusters of glands, resembling in appearance and

situation those described in the human stomach, but upon a larger scale and more obvious to the naked eye. Beyond this part, the internal membrane of the cardiac portion has one uniformly smooth surface studded over with glands, particularly along the great curvature. The pyloric portion has a number of glands of a different structure more minute than those of the cardiac portion; these are placed principally along the surface of the small arch, and are continued on to the pylorus, where there is a zone of glands surrounding the termination of the stomach, and the origin of the duodenum.

In this animal these glandular structures are very conspicuous, which are not to be detected in the domesticated carnivorous animals. Vide Plate XII. fig. 1.

XVIII. *The Vampyre Bat.*

The animal from which the stomach was taken was nine inches long, the extent of its wings 36 inches.

The œsophagus swells out before it enters the general cavity, and that dilatation from its internal structure appears to belong to the stomach, as there is no contraction, or distinct orifice beyond it. To the left of the œsophagus there are two dilatations with a neck between them; the furthest of these has a smooth surface and the coats are very thin; in the other there are several deep longitudinal rugæ, some of which are continued into similar rugæ, or bands in the dilated portion of the œsophagus. This portion of the stomach has more the appearance of an appendix than belonging to the general canal. There are six rugæ or bands in the œsophageal portion, four of which are continued towards the pylorus, giving a direction to the food in that course. After the stomach

has extended the same length on the left of the œsophagus as on the right, it is turned back upon itself, as far as the entrance of the œsophagus, then makes another turn, and ends in the pylorus by a very small valvular opening, which scarcely gives a passage to air when in a contracted state. No part of the stomach is lined with a cuticle, and a quarter of an inch from the pylorus, there is an appearance of glandular structure; this is very faint in consequence of the animal having been long kept in spirits. This animal occasionally feeds on flowers, since stamina resembling those of Eugenia, were found filling up several portions of the stomach, the filamentæ and antheræ in a perfect state. Vide Plate XI. fig. 3.

XIX. *Long-eared Bat.*

The animal from which the stomach was taken was $2\frac{1}{4}$ inches long, and the tail included in the web about two inches. The wings extended nine inches. The œsophagus is extremely small, and lined with a cuticle which termates at the orifice of the stomach. The general cavity is distinctly divided into a cardiac and pyloric portion: close to the pylorus, and surrounding it, there is an appearance of small glands.

In the spectre bat there is a swell in the œsophagus as in the vampyre; so that in the different species of bats the stomachs vary very much from one another. Vide Plate XI. fig. 2.

XX. *The Hawk.*

The stomach is a direct continuation of the œsophagus, distinguished from it by the termination of the cuticular lining, and by the solvent glands having their origin at that part. These are disposed in four longitudinal ridges; they appear

to be made up of an infinite number of small white tubes, the direction of which is perpendicular to the internal membrane. The cavity of the stomach is divided into two portions; the cardiac is the largest, forms the general cavity, and when in a contracted state, has a rugous internal surface: the pyloric is small, and projects from one side of the other. Immediately beyond the pylorus the duct of the liver opens. Vide Plate XII. fig. 2 and 3.

XXI. *The Cormorant.*

The stomach is formed on the same general principle as that of the hawk; but the solvent glands differ so much in their appearance from those of other birds of prey, that I have represented them in the annexed drawing. The cuticular lining of the œsophagus terminates at the orifice of the stomach, and several openings of the œsophageal glands are seen at that part. Immediately within the stomach are situated the solvent glands, forming two circular projecting surfaces, each of them $1\frac{5}{8}$ inch in diameter, covered with small orifices like pin holes, which extend into the substance of the gland. The communication with the pyloric portion is on one side just below these solvent glands. Vide Plate X. fig. 2 and 3.

XXII. *The Viper.*

The stomach is a continuation of the œsophagus: its origin is distinguished by the termination of the cuticular lining of that canal, the coats becoming thicker at that part, and the inside being surrounded by a zone of solvent glands. The cardiac portion terminates in a small orifice at its lower part, and the pyloric is through its whole extent not wider than this orifice; its internal membrane has longitudinal folds; but

immediately beyond the pylorus, where the duct of the liver opens, the duodenum has a different structure. Vide Plate XIII. fig. 1.

XXIII. *The Turtle.*

The stomach is a continuation of the œsophagus, and begins where the projecting papillæ of that canal terminate. The cardiac portion is of an oval form, has a rugous internal surface, but no distinct glands were observed; it communicates with, the pyloric by a small orifice; the pyloric is bent upwards and retained in that situation by the mesogaster; its coats are very thick, and their substance contains many small glands with ducts leading into its cavity. Vide Plate XIII. fig. 2.

XXIV. *The Frog.*

The stomach is in its general characters like that of the turtle, but on a very small scale, and no particular glandular structures were distinguished. Vide Plate XIII. fig. 3.

XXV. *The Blue Shark.*

The œsophagus is three inches long, and lined with a cuticle. The cardiac portion of the stomach is 18 inches long and 8 in diameter; it communicates with the pyloric by an orifice $\frac{1}{8}$ of an inch in diameter. The pyloric portion is 18 inches long and one inch in diameter. The fish was 7 feet 5 inches long.

The internal membrane of the cardiac portion is rugous, and orifices leading to glands are seen upon it. That of the pyloric portion is smooth. Beyond the pylorus there is an enlargement where the intestine begins, and into this cavity the duct of the liver opens. Beyond this the spiral valve of

the gut has its origin. The spleen surrounds both the cardiac and pyloric portion, and is represented in the engraving. Vide Plate XIII. fig. 4.

The description of the stomach of the cod fish by mistake is placed the second of these descriptions.

Observations on the Stomachs which have been described.

In the stomachs of ruminating animals, the processes the food undergoes before it is converted into chyle, are more complex than in any others. It is cropped from the ground by the fore teeth, then passes into the paunch, where it is mixed with the food in that cavity; and it is deserving of remark, that a certain portion is always retained there; for although a bullock is frequently kept without food seven days before it is killed, the paunch is always found more than half full; and as the motion in that cavity is known to be rotatory by the hair balls found there being all spherical or oval with the hairs laid in the same direction, the contents must be intimately mixed together; the food is also acted on by the secretions belonging to the first and second cavities; for although they are lined with a cuticle, they have secretions peculiar to them. In the second cavity these appear to be conveyed through the papillæ, which in the deer are conical (Vide Plate V. fig. 3), and when examined by a lens whose focus is $\frac{1}{2}$ inch, they are found to have three distinct orifices, and that part of each papilla next the point is semitransparent. These secretions are ascertained by Dr. STEVENS's experiments to have a solvent power in a slight degree, since vegetable substances contained in tubes were dissolved in the paunch of a sheep.*

* *Dissertatio Physiologica inauguralis de Alimentorum concoctione. Autore* EDWARDO STEVENS, Edinb. 1777.

The food thus mixed is returned into the mouth, where it is masticated by the grinding teeth; it is then conveyed into the third cavity, in which it would appear from the gas * let loose, that a decomposition takes place, and thence it is received into the upper portion of the fourth cavity.

The changes which are produced on the food in the three first cavities are only such as are preparatory to digestion, and it is in the fourth alone that process is carried on. In the plicated portion the food is acted on by the secretion of the solvent glands; and in that portion of the fourth cavity of the deer's stomach small orifices are seen in the internal membrane leading to cavities, the size of a pin's head, which I consider to be the openings of these glands, since they bear some resemblance to those of other stomachs. In the lower portion the formation of chyle is completed.

In birds with gizzards the food goes through very similar changes; it is picked up by the bill, which in the smaller birds separates the husk from the seed; it then passes into the crop, where it is acted on by the secretions of that cavity, after which it is received into the gizzard, to undergo the same change produced by the grinding teeth of the ruminants; the secretion of the solvent glands is then poured upon it, acting upon the nutritious part before it is spread upon the glandular structure at the orifice of the gizzard, in which last situation it is formed into chyle.

In the whale tribe, the first cavity, although lined with a cuticle, has secretions peculiar to it, and therefore corresponds with the first and second of the ruminants, and with the crops of birds with gizzards: it answers however a further

* Mr. Davy and Mr. W. Brande examined this gas, and found it to be inflammable, and not to contain carbonic acid; which establishes a difference between this process, and fermentation.

purpose, by dissolving its contents sufficiently to prevent the necessity of rumination, or the use of a gizzard. The second cavity performs the same office as the plicated portion of the fourth cavity of the ruminant, and the fourth is that in which the chyle is formed. This complex structure of the stomach in the whale tribe, although it gives it an appearance of great similarity to that of the ruminant, is not at all formed on the same principle, since the additional cavities in the ruminant are to prepare the food for the process of digestion; while in the whale tribe no such preparation is required; but as the fishes they feed upon are swallowed whole, and have large sharp bones which would injure any surface not defended by cuticle, a reservoir became necessary, in which they may be dissolved and converted into nourishment, without retarding the digestion of the soft parts. The very narrow communication between the second, third, and fourth cavities, resembles the opening between the cardiac and pyloric portion in fishes.

The stomachs of this tribe of animals are therefore introduced here, as being next in order with respect to the complexity of parts, and having by the division of them led me to the present investigation, although it is by no means their proper place, with respect to their mode of digestion.

The animals, nearest allied to the ruminants in their mode of digestion, are those which, like them, retain a portion of food in the cardiac extremity of the stomach, that it may undergo a change, before it is submitted to the action of the solvent liquor; and when so hard as to render it necessary, return it again into the mouth, to be masticated a second time.

The hare and rabbit are of this kind; the cardiac portion of the stomach is never completely emptied, and they occa-

sionally ruminate. In proof of both these facts, a rabbit, which had been seven days without food, died, and the cardiac portion of the stomach was found to contain more than half of its usual quantity of contents: they were rather softer than common, and a number, amounting to 50 or 60 of distinctly formed pellets the size of shot, were collected together in the cardiac extremity, immediately below the œsophagus. These could not have been formed at the time of eating, since in seven days, the action of the stomach would have destroyed their shape. They must therefore have acquired it by the animal chewing the cud.

This second class of ruminants have no cuticular lining to their stomachs, which may arise from their being more cautious feeders than the others, so that they are not liable to receive into the stomach any thing which can injure its internal membrane. All that portion of the stomach, which corresponds with the first cavity in the true ruminant, has one uniform structure, and is covered by a viscid mucus, but beyond this there are orifices, which I believe belong to solvent glands of a very small size; and towards the pylorus, the glandular appearance is of a different kind; so that in these stomachs the changes the food goes through correspond very closely with those it undergoes in ruminants.

The next order of animals with respect to digestion consists of the beaver and dormouse. These, both in the shape and general appearance of the stomach, as well as of the teeth, bear a close affinity to the hare; but they have a glandular structure peculiar to them, which seems to correspond with the solvent glands of other animals; and as the dormouse empties

its stomach completely, there is reason to believe that the beaver does so likewise, and that neither of them ruminate, since the regurgitation of the food would be attended with difficulty from the situation of these glandular structures ; and it is probable, as they do not ruminate, the increased secretion of a solvent liquor renders it unnecessary.

The changes the food undergoes in these stomachs are only two; it is acted on by the secretion from the solvent glands, and afterwards converted into chyle by the secretion of those near the pylorus. This is a less complex process than in many of the stomachs not yet taken notice of, and is exactly similar to what takes place in carnivorous animals; it may therefore be considered as a connecting link between the ruminating and carnivorous stomachs.

After these, which form a regular series from the ruminants, are the stomachs with cuticular reservoirs, in which the food is macerated, before it is submitted to the process of digestion. Animals of this kind are the water-rat, in which there is a permanent division between the cuticular cavity and the digestive part of the stomach ; the common rat and the mouse, in which there is only a muscular one. The cuticular lining is thick and impervious ; beyond it is a glandular part, that secretes a mucus found adhering to its surface ; and further on are orifices, which appear to belong to the solvent glands. These animals do not ruminate, and there is a kind of provision in nature to prevent regurgitation of the food. When kept without food for several days they completely empty their stomachs.

The horse and the ass, although animals in all other res-

pects different, correspond so very closely in the structure of their stomachs with the rat and mouse, that their stomachs must be considered of the same kind.

In these the food is rendered easy of solution by remaining in the cuticular reservoirs; it is then acted on by the solvent liquor, and in the pyloric portion converted into chyle.

The stomach of the kanguroo, from the peculiarities of its structure, forms an intermediate link between the stomachs of animals which occasionally ruminate, those which have a cuticular reservoir, and a third kind not yet noticed, with processes or pouches at their cardiac extremity, the internal membrane of which is more or less glandular. The kanguroo is found to ruminate, when fed on hard food. This was observed by Sir Joseph Banks, who has several of these animals in his possession, and frequently amused himself in observing their habits. It is not however their constant practice, since those kept in Exeter Change have not been detected in that act. This occasional rumination connects the kanguroo with the ruminant. The stomach having a portion of its surface covered by cuticle, renders it similar to those with cuticular reservoirs; and the small process from the cardia, gives it the third distinctive character; indeed it is so small, that it would appear placed there for no other purpose.

The kanguroo's stomach is occasionally divided into a greater number of portions than any other, since every part of it, like a portion of intestine, can be contracted separately; and when its length, and the thinness of its coats are considered, this action becomes necessary to propel the food from one extremity to the other. Such a structure of stomach makes regurgitation of its contents into the mouth very easily per-

formed. The food in this stomach goes through several preparatory processes; it is macerated in the cuticular portion; it has the secretion from the pouch at the cardia mixed with it; and is occasionally ruminated. Thus prepared, it is acted on by the secretion of the solvent glands, which probably are those met with in clusters in the course of the longitudinal bands and afterwards converted by the secretions near the pylorus into chyle.

The animals, whose stomachs have processes or pouches at their cardiac extremity, are the kanguroo, hog, pecari, hippopotamus, and elephant.

The pecari's stomach bears the nearest resemblance to those with cuticular reservoirs, having a portion of its surface lined with cuticle; but it only extends to a small distance from the termination of the œsophagus, and is not continued over any part of the great curvature.

The hippopotamus's stomach I have never seen, and DAUBENTON's description and engravings are taken from that of a fœtus; so that the structure of its minute parts is imperfectly known; but there is no doubt of there being a large pouch on each side of the cardiac portion, and there is reason to believe that no part of the cavity of the stomach is lined with cuticle.

The elephant's stomach is the most simple of this kind. It has no cuticular lining; the elongation at the cardia is only a continuation of the general cavity, distinguished from it by the membranous septa; and the broad one may act as a valve, and occasionally preclude the food from passing.

In these stomachs the pouches at the cardia can only be connected with the preparation of the food, softening it by means of their secretions, or retaining it within their cavities;

the other glandular structures are similar to those in the ass and rat, only more conspicuous.

It is deserving of remark, that the internal structure of the stomachs fitted for digesting vegetable substances, corresponds much less with the kind of teeth, than it has been generally supposed to do. The animals with chissel teeth have no uniformity in the structure of their stomachs; those of the beaver and dormouse being of one kind; the hare's and rabbit's of another; the squirrel's of a third, resembling that of the monkey; the guinea pigs of a fourth, differing from that of the squirrel, in there being a greater disproportion between the thickness of the coats of the cardiac and pyloric portions; the rat tribe of a fifth, which resembles the stomach of the horse and ass, animals whose teeth have a very different form.

On the other hand, all the ruminants with horns have one structure of stomach; all those with fighting teeth another, as has been observed in a former paper; also all the animals with projecting tusks have the pouches at the cardia, which appear to be peculiar to them, although there is no connexion we yet know of between these weapons of defence and the stomach.

As the elephant's grinding teeth are the best fitted for preparing vegetable food for digestion, so the stomach in its structure approaches nearer to those of carnivorous animals.

The stomachs whose structure has been hitherto considered belong to animals that feed on vegetables, and chiefly on the leaves, roots, and branches of plants. In the gradation towards carnivorous stomachs, we are next to take notice of those that belong to animals whose principal food is the fruits of trees, which appear to require less preparation for the process

of digestion; of this kind are the stomachs of the squirrel and monkey. These in their general appearance resemble very closely the human stomach; at least the few opportunities which have occured to me of examining them, have not enabled me to detect any circumstances in which they differ.

The human stomach appears to be the uniting link between those that are fitted only to digest vegetable substances, and those that are entirely carnivorous; and yet we find in its internal structure it is in every material respect similar to those of the monkey and squirrel, which only digest vegetable productions, and also equally similar to those of carnivorous animals. From this it would appear that many parts of vegetables are as easily digested as animal substances, and require the same organs for that purpose; but others again require a particular preparation, without which they cannot be converted into chyle; of these last the principal are the grasses, which the human stomach is unable to digest.

The human stomach is divided into a cardiac and pyloric portion, by a muscular contraction similar to those of other animals; and as this circumstance has not before been taken notice of it, may be necessary to be more particular in describing it.

The first instance, in which this muscular contraction was observed in the human stomach, was in a woman, who died in consequence of being burnt. She had been unable to take much nourishment for several days previous to her death. The stomach was found empty, and was taken out of the body at a very early period after death. It was carefully inverted to expose its internal surface, and gently distended with air. The appearance it put on has been already described. The con-

traction was so permanent, that after the stomach had been kept in water for several days in an inverted state, and at different times distended with air, the appearance was not altogether destroyed.

Since that time I have taken every opportunity of examining the human stomach recently after death, and find that this contraction in a greater or lesser degree is very generally met with. The appearance which it puts on varies: sometimes it resembles that of the ass (Vide Plate VIII.) so that this effect is not produced by a particular band of muscular fibres, but arises from the muscular coat in the middle portion of the stomach being thrown into action: and this for a greater or lesser extent, according to circumstances. When this part of the stomach is examined by dissection, the muscular fibres are not to be distinguished from the rest.

If the body is examined so late as 24 hours after death, this appearance is rarely met with, which accounts for its not having before been particularly noticed.

PERRAULT found a contraction somewhat similar in a lion's stomach, which appeared to him extraordinary, as it was only met with in one instance out of four, that were examined. He gives a drawing of the appearance, but makes no comments on the cause of the contraction.*

* La conformation du ventricule étoit particulière, et bien différent en ce sujet de celle, que nous avons trouvés aux autres lions, que nous avons dissequés, où le ventricule étoit semblable à celui des chiens et des chats; ayant un fond ample et large vers l'orifice supérieur qui alloit toujours en s'étrecissant vers le pylore; mais celui-ci avoit le fond separé en deux, en quelque façon comme les animaux qui ruminent. Ce forme particulière du ventricule n' étoit qu'en un seul des quatre animaux de cette espèce que nous avous dissequés, sçavoir deux lions et deux lionnes.

Mémoires pour servir à l'Histoire Naturelle des Animaux, dressez par M. PERRAULT, Fol. Ed. 1676.

Finding this contraction was met with, when the human stomach was nearly empty, I endeavoured to produce it in the cat, by having the stomach emptied by means of an emetic a short time before the animal's death. This did not however succeed; for although in the contracted state the line between the cardiac and pyloric portions was very distinct, and the last more contracted than the former, yet upon distending the stomach with air, the middle portion relaxed equally with the rest. The contraction at this part is therefore only to be seen, when these fibres have acted independently of the others; which takes place while the functions of the stomach are going on, but cannot be artificially produced.

In examining the stomach of a dog in a contracted state, and afterwards when it was distended, the line between the two portions could be distinctlyperceived, even after the contraction was destroyed, by the longitudinal folds of the internal membrane of the pyloric portion all terminating there.

That the food is dissolved in the cardiac portion of the human stomach, is proved by that part only being found digested after death; the instances of which are sufficiently numerous to require no addition being made to them. This could not take place unless the solvent liquor was deposited there. Mr. HUNTER goes so far as to say, in his paper on this subject, "there are few dead bodies in which the stomach at its great "end is not in some degree digested."

That the chyle is not formed there, and also that it is completely formed before the food passes through the pylorus, is proved by the result of some experiments of Mr. HUNTER's made upon dogs in the year 1760; and as they were instituted for a very different purpose,—that of determining whe-

ther the gastric juice is acid or alkaline,—the results were detailed without any possible bias.

The stomachs of seven dogs were examined immediately after death, which took place while digestion was going on; and among other observations the following appear among Mr. HUNTER's notes made at the time:

"In all the dogs the food was least dissolved or even mixed "towards the great end of the stomach, but became more and "more so towards the pylorus; and just within the pylorus, "it was mixed with a whitish fluid like cream, which was also "found in the duodenum."

He afterwards adds; "It is plain that digestion is com- "pleted in the stomach, as none of the crude food is found "beyond that cavity; and even within the pylorus there is the "same white fluid that is met with in the duodenum."

From the result of these experiments, as well as from the analogy of other animals, it is reasonable to believe that the glands situated at the termination of the cuticular lining of the œsophagus, which have been described, secrete the solvent liquor, which is occasionally poured on the food, so as to be intimately mixed with it before it is removed from the cardiac portion: and the muscular contraction retains it there till that takes place.

Such contraction being occasionally required in the stomach, accounts for its being more or less bent upon itself, which renders it more readily divided into two portions by the action of the muscular fibres at that part where the angle is formed.

It accounts for men occasionally ruminating, a process, which without such a contraction, could hardly take place. That some

men ruminate, the accounts of authors are sufficiently explicit to put beyond all doubt; particularly the instances collected by PEYER from FABRICIUS *ab aquapendente* and others, as well as from his cotemporaries, in all six or seven instances. Of these, two were examined after death. In one of them the œsophagus was unusually muscular, but nothing particular was met with in the stomach: in the other, nothing is said of the œsophagus, but the internal surface of the stomach was very rough.

The fact, however, does not rest on these authorities, since a case of this kind has come within my own observation.

The instance to which I allude, is a man 19 years of age, blind, and an ideot from his birth, who is now alive. He is very ravenous, and they are obliged to restrict him in the quantity of his food, since, if he eats too much, it disorders his bowels. Fluid food does not remain on his stomach, but comes up again. He swallows his dinner, which consists of a pound and a half of meat and vegetables, in two minutes, and in about a quarter of an hour he begins to chew the cud. I was once present on this occasion. The morsel is brought up from the stomach with apparently a very slight effort, and the muscles of the throat are seen in action when it comes into the mouth; he chews it three or four times, and swallows it; there is then a pause, and another morsel is brought up. This process is continued for half an hour, and he appears to be more quiet at that time, than at any other. Whether the regurgitation of the food is voluntary or involuntary, cannot be ascertained, the man being too deficient in understanding to give any information on the subject.

This contraction of the stomach also explains the circum-

stance of its contents not being completely discharged, by the first effect of an emetic, which only empties the cardiac portion: the contraction preventing the pyloric portion from being emptied till the violence of the straining ceases, at which time relaxation takes place.

It may also enable us to account for many symptoms that occur in the diseases of this organ, particularly the violent cramps, to which it is liable: as from the situation of the pain they probably arise from preternatural contractions of these muscular fibres. On the other hand, the indigestion met with in debilitated stomachs may proceed from this part having lost its proper degree of action, and therefore the food is not retained in it so as to be acted on by the different secretions.

This however is not the place to enter into these subjects; the object of the present investigation has been to collect facts in comparative anatomy, that may throw light upon the conversion of the food into chyle, and to abstain as much as possible from all matters of opinion;—no easy forbearance in going over ground, that has given rise to so many theories, and which the mind cannot contemplate, without forming a variety of conjectures.

The stomach of the truly carnivorous quadruped appears to be made up of the same parts as the human. In the lynx, the different structures are more strongly marked, the solvent glands are more conspicuous, the pyloric portion is more bent, which renders the division between it and the cardiac more distinct, the muscular coats of the pyloric portion are much stronger, and on its internal surface, glands are very obvious which are not to be observed in the human.

The stomachs of some carnivorous animals have glandular

structures peculiar to them; these are in the pyloric portion; there are also similar glands in the stomachs of some graminivorous animals, as has been already explained. The following may be mentioned as instances of this kind.

In the lynx, a glandular zone surrounds the orifice of the pylorus.

In the mole, there is a similar zone.

In the stoat, and armadillo, there is a glandular structure near the pylorus.

In the sea otter, there is a glandular structure extending from the pyloric portion into the duodenum, described in a former paper.

In tracing the gradation from carnivorous quadrupeds to birds of prey, it would have been natural to expect that the bat, which has wings, and lives on animal food, should form an intermediate link: this, however, is not the case; the stomach of the long-eared bat resembles those of small carnivorous quadrupeds; that of the vampyre bat, which will be found to live on vegetables, has more the appearance of an intestine, and may, from its form, be mistaken for the cæcum and colon; in this repect it approaches the kanguroo, and still more closely the kanguroo rat; its cardiac portion is shorter, and its pyloric longer, than in the stomach of that animal, and there is no valvular structure at the orifice of the cardia. I have mentioned these differences as there is no engraving of the kanguroo rat's stomach annexed to the present Paper.

The only real link between the stomachs of quadrupeds and birds is that of the ornithorinchus, which, however, is more an approach to the gizzard, being lined with a cuticle,

containing sand, and having the same relative situation to the œsophagus and duodenum. The food of this animal is not known; it is probably of both kinds; the papillæ at the pylorus, which appear to be the excretory ducts of glands, are peculiar to it.

The stomachs of birds of prey are formed upon the same principle as those of carnivorous quadrupeds, but their cavity is more a continuation of the œsophagus, and the solvent glands are more conspicuous and numerous. Both these differences may be accounted for from their swallowing their prey whole, or nearly so; which requires a more direct passage into the stomach, and a greater quantity of secretion from the solvent glands, than when the food has undergone mastication. The cardiac portion of these stomachs is very distinct from the pyloric.

In snakes, turtles, and fishes, the stomachs have the same characters as in birds of prey, but the cardiac and pyloric portions are still more distinct from each other, and the solvent glands are in general distributed over a larger surface of the cardiac portion.

From the series of facts and observations which have been adduced, the following conclusions may be drawn.

That the solvent liquor is secreted from glands of a somewhat similar structure in all animals, but much larger and more conspicuous in some than others.

That these glands are always situated near the orifice of the cavity whose contents are exposed to their secretion.

That the viscid substance found on the internal membrane, of all the stomachs that were examined recently after death, is reduced to that state by a secretion from the whole surface

of the stomach which coagulates albumen. This appears to be proved, by every part of the fourth cavity of the calf's stomach having the property of coagulating milk.

This property in the general secretion of the stomach, leads to an opinion, that the coagulation of fluid substances is necessary for their being acted on by the solvent liquor; and a practical observation of the late Mr. HUNTER, that weak stomachs can only digest solid food, is in confirmation of it.

That in converting animal and vegetable substances into chyle, the food is first intimately mixed with the general secretions of the stomach, and after it has been acted on by them, the solvent liquor is poured upon it, by which the nutritious part is dissolved. This solution is afterwards conveyed into the pyloric portion, where it is mixed with the secretions peculiar to that cavity, and converted into chyle.

The great strength of the muscles of the pyloric portion of some stomachs, will, by their action, compress the contents, and separate the chyle from the indigestible part of the food.

In animals whose food is easy of digestion, the stomach consists of a cardiac and pyloric portion only; but in those whose food is difficult of digestion, other parts are superadded, in which it undergoes a preparation before it is submitted to that process.

EXPLANATION OF THE PLATES.

(PLATE V.)

Fig. 1. Represents the gizzard of a turkey, with a portion of the œsophagus and duodenum attached to it. The œsophagus and duodenum are both laid open to expose different glandular structures, but the gizzard itself is entire.

a. The œsophagus immediately below the crop covered with a cuticle.

b. The openings of the solvent glands placed on a surface which has no cuticular covering.

c. Horny ridges between the solvent glands and the lining of the gizzard.

d. A minutely granulated surface between the cavity of the gizzard and the duodenum.

e. The inner surface of the duodenum.

Fig. 2. Shows the internal surface of the stomach of the cod fish.

a. The inner surface of the œsophagus lined with a cuticle, having small processes projecting from it.

b b. The cardiac portion of the stomach, the inner membrane of which is soft and rugous, forming longitudinal folds, and having orifices of glands upon the projecting ridges.

c. The pyloric portion.

d. The pylorus.

Fig. 3. Represents a small portion of the inner surface of the second cavity of the deer's stomach, showing the shape of the cells and the form and situation of the papillæ.

Fig. 4. Represents the hare's stomach inverted, to show its natural form, and the appearance of the different parts of its internal membrane.

a. The œsophagus.

b b. The cardiac portion.

c. A muscular band separating the cardiac from the pyloric portion.

d d. The pyloric portion.

e. A glandular appearance believed to be the solvent glands.

f. The pylorus.

g. The duodenum.

Fig. 5. Is an external view of the rabbit's stomach distended with air, to show the muscular coat; the fibres are uniformly of the same strength over the whole of the cardiac portion, but where the pyloric portion begins they are stronger, and continue so half way to the pylorus, at which part there is a circular band, and beyond it the fibres become spiral to the pylorus; the layers of spiral fibres decussate one another.

(PLATE VI.)

Fig. 1. Is a view of the beaver's stomach inverted, to show its shape and the appearance of the internal membrane.

a. The œsophagus.

b b. The cardiac portion.

c. The glandular structure peculiar to this stomach, which appears to be a cluster of solvent glands.

d. The contraction between the cardiac and pyloric portion.

e. The pyloric portion.

f. A glandular zone at the pylorus.

g. The duodenum.

Fig. 2. Represents the orifices of the glandular structure, to show how much they admit of being dilated, and that in that state they expose three internal openings leading into the substance of the gland.

a. The œsophagus.

b b. The three ridges of glandular structure.

Fig. 3. Shows the different processes which belong to two of the three internal openings of the gland.

Fig. 4. An external view of the stomach of the dormouse, to show its peculiar glandular structure at the termination of the œsophagus, and the cardiac and pyloric portions of the stomach.

a. The glandular structure in the œsophagus.

b. The cardiac portion of the stomach.

c. The pyloric portion.

Fig. 5. The dormouse's stomach laid open to expose its internal surface.

a. The orifices of the gland corresponding to those of the beaver.

b b. The two corresponding parts of the cardiac portion of the stomach.

c. The pyloric portion.

Fig. 6. Is a view of the stomach of the water-rat inverted, to show its internal structure.

a. The œsophagus.

b b. The cardiac portion covered with cuticle.

c. A process of cuticle on each side extending into the pyloric portion.

d. A glandular structure.

e. Orifices of glands believed to secrete the solvent liquor.

f. The pylorus.

g. The duodenum.

(PLATE VII.)

A view of the ass's stomach inverted, to show its internal surface.

a. The œsophagus.

b b. The cardiac portion lined with cuticle, the termination of which is distinctly seen.

c c. A glandular structure.

d. The orifices of solvent glands.

e. The pylorus.

(PLATE VIII.)

An internal view of the stomach of the kanguroo, which exposes one half of its cavity.

a. The œsophagus.

b. Its termination in the stomach.

c c. The surface covered with cuticle.

d. The process at the cardia, which is glandular.

e e. The termination of the cuticular lining.

f f. The longitudinal band.

g g. The beginning of the clusters of glands which appear to secrete the solvent liquor.

h. The cavity at the pylorus.

i. The pylorus.

k. The duodenum.

(PLATE IX.)

Represents an internal view of the hog's stomach by inverting its cavity.

a. The œsophagus.

b b. The surface covered with cuticle.

c. The process at the cardia.

d d. The surface of the cardiac portion.

e e. The massy glandular substance between the cardiac and pyloric portions.

f f. The orifices of solvent glands.

g g. The pyloric portion.

h. The pylorus.

(PLATE X.)

Fig. 1. Represents a section of the elephant's stomach, to show its internal structure, taken from a dried preparation, in which the blood vessels had been injected and the cavity afterwards blown up.

a. The œsophagus.

b b. The portion at the cardia, in which the transverse folds are met with : five of these are broad, and nine narrow.

c c. The cardiac portion.

d. The pyloric portion.

e. The pylorus.

Fig. 2. Shows the external appearance of the cormorant's stomach.

a. The œsophagus.

b. The cardiac portion.

c. The pyloric portion.

Fig. 3. Is the stomach laid open, and the internal parts exposed, particularly the solvent glands, the appearance of which differs from that of other birds of prey. On one portion the orifices are seen empty, on the other they are covered with mucus in a coagulated state.

(PLATE XI.)

Fig. 1. The human stomach inverted, to show its internal surface.

a. The œsophagus with its cuticular covering, and the orifices of the œsophageal glands, which are most conspicuous just above the termination of the cuticular lining.

b b. The cardiac portion.

c. The solvent glands.

d. The contraction, dividing the cardiac from the pyloric portion.

e. The pyloric portion.

f. The pylorus.

Fig. 2. A longitudinal section of the vampyre bat's stomach, to shew its internal surface.

Fig. 3. The stomach of the long-eared bat inverted, to show its internal surface.

(PLATE XII.)

Fig. 1. The lynx's stomach inverted, to show its internal surface.

a. The œsophagus, in which the internal membrane is thrown into folds in a transverse direction.

b b. The cardiac portion.

c. The solvent glands.

d. The pyloric portion.

e. The pylorus.

f. The duodenum.

Fig. 2. The external appearance of the stomach of the hawk.

a. The œsophagus.

Fig. 1.

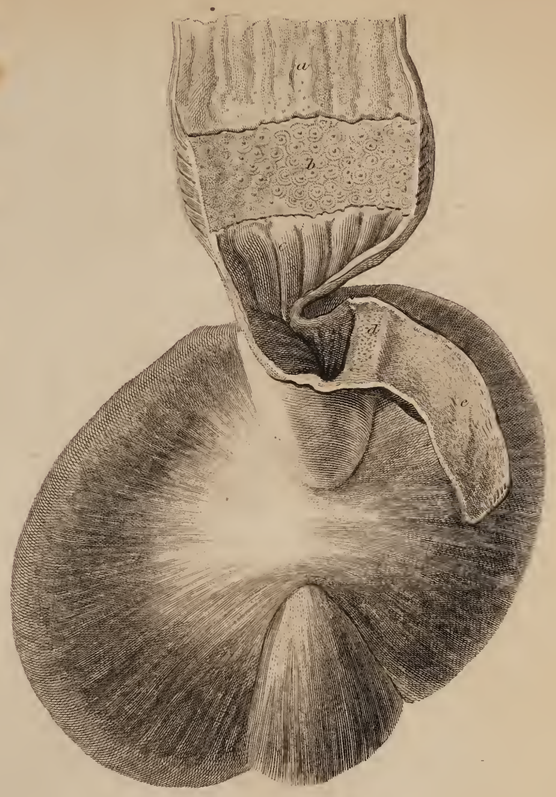


Fig. 3.

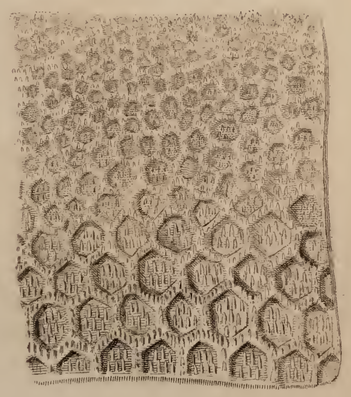


Fig. 2.



Fig. 4.



Fig. 5.



W. Clift del.

Basire Sc.

Fig. 2.

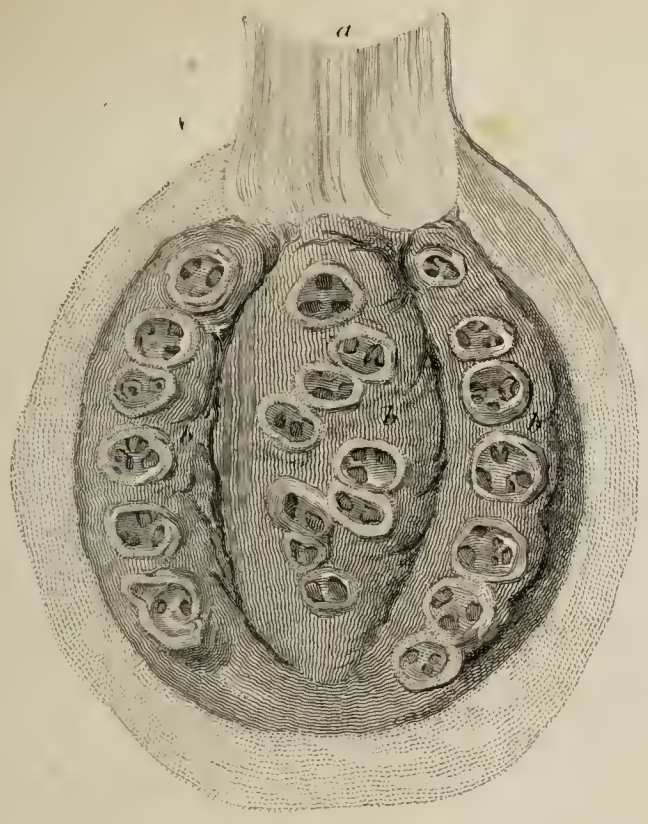


Fig. 4.

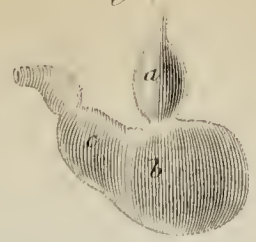


Fig. 5.

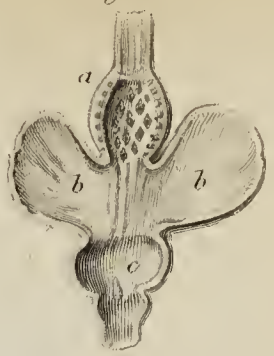


Fig. 6.

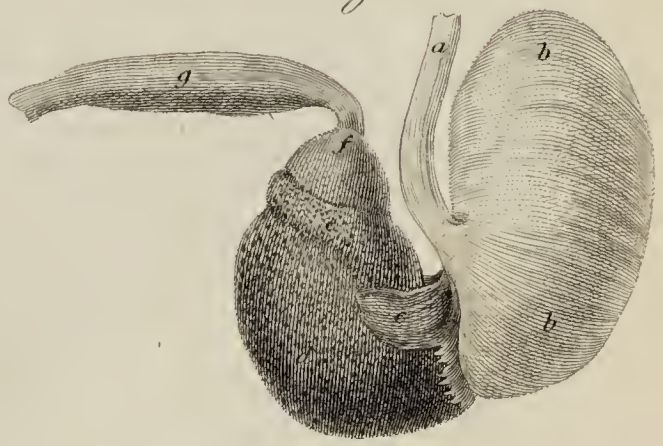
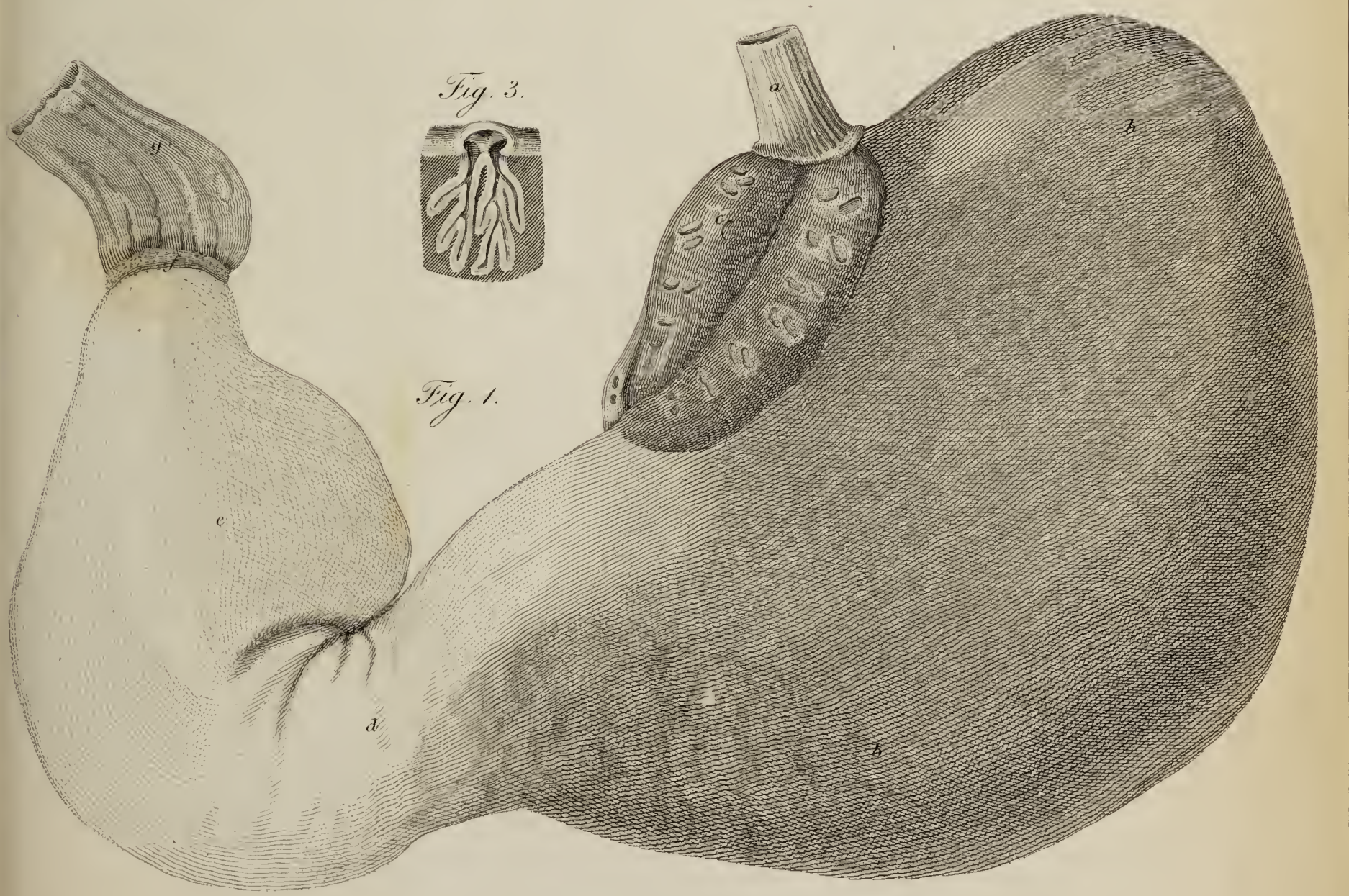
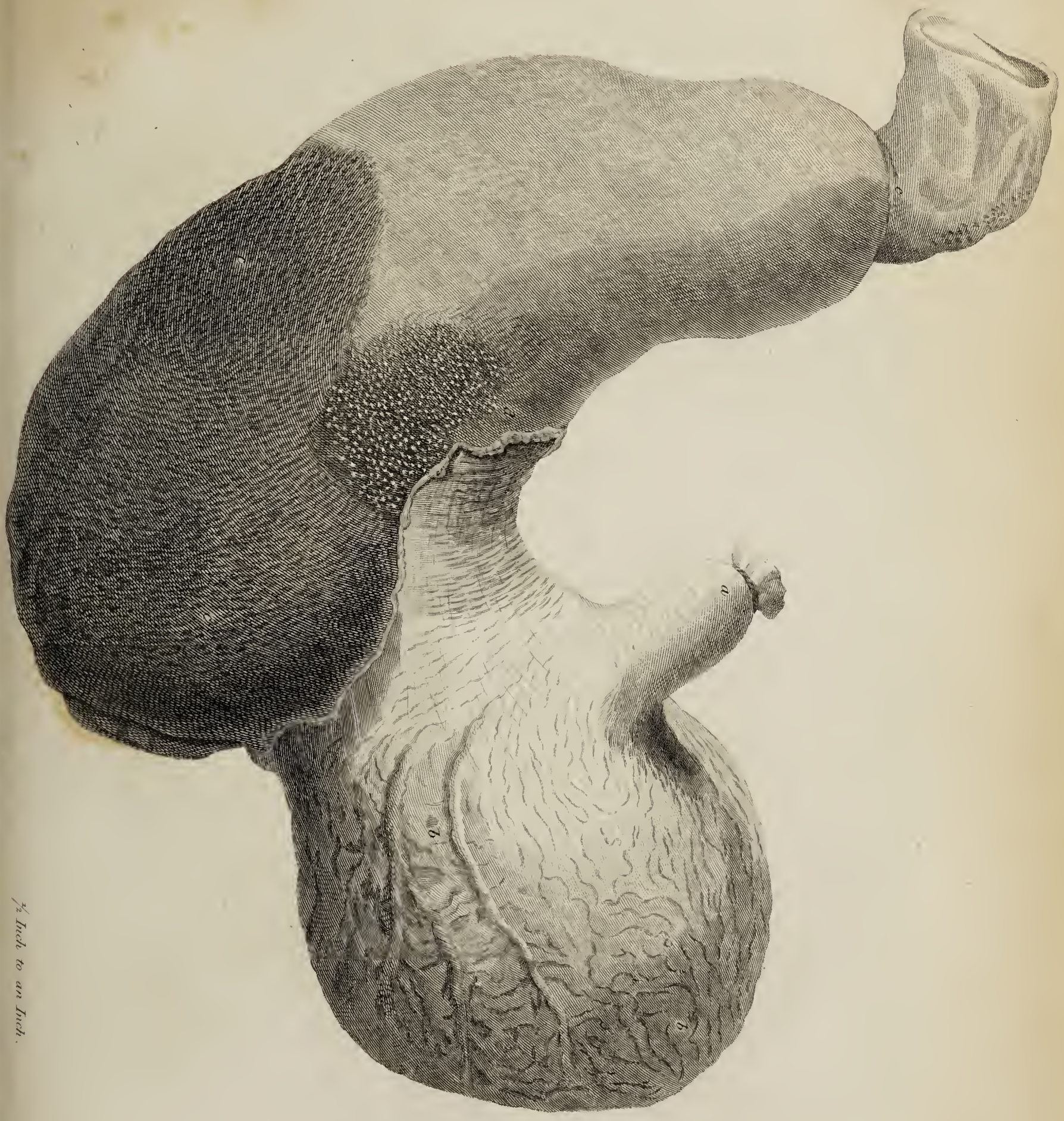


Fig. 3.

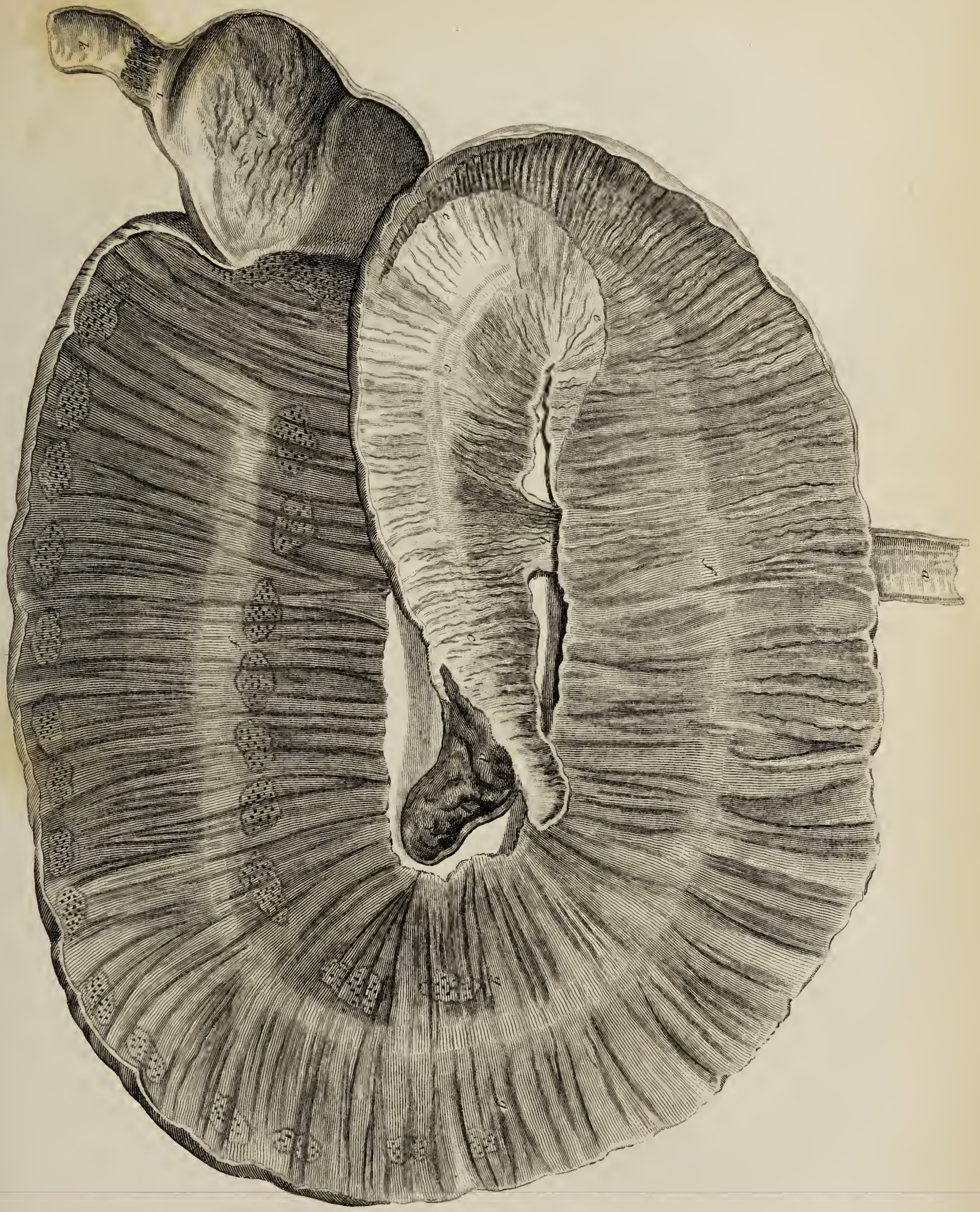


Fig. 1.





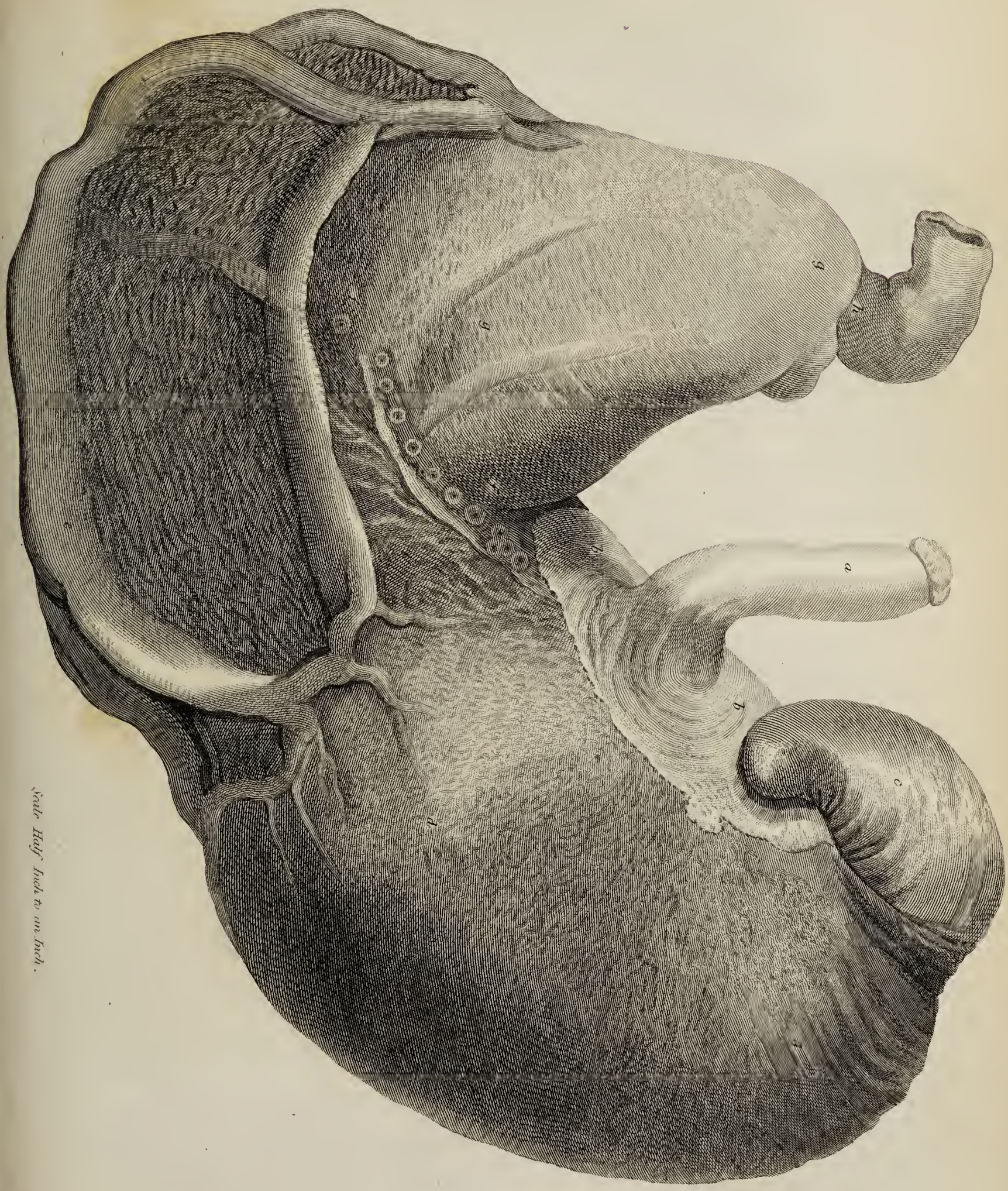
½ Inch to an Inch.



Scale Half Inch to an Inch.

W. Clift del.

Basire Sc.



Scale Half Inch to an Inch.

Clift del. *Basire Sc.*


a
c
Fig. 1.
½ Inch to an Inch.
Fig. 3.
a
c
b
Fig. 2.
Scale 4 Inches to 12.


Fig. 1.
Fig. 2.
Fig. 3.



Fig. 3.



Fig. 2.



Fig. 1.

¼ Inch to an Inch.

Fig. 4.



Fig. 2.



Fig. 1.



Fig. 3.



Half an Inch to an Inch.

b b. The cardiac portion.

c. The pyloric portion.

Fig. 3. The internal appearance.

a. The œsophagus.

b. The solvent glands.

c c. The cardiac portion.

d. The pyloric portion. At the beginning of the duodenum is the opening of the duct of the liver.

(PLATE XIII.)

Fig. 1. The stomach of the viper laid open, to show its internal surface.

Fig. 2. The stomach of the turtle, exposing one half of its cavity.

Fig. 3. The stomach of the frog, exposed in the same manner.

Fig. 4. A longitudinal section of the stomach of the blue shark. This fish was caught at Hastings, and purchased for me by my friend Col. Bothwell of the Scotch Greys, who happened to be there.

It is proper to remark, that previous to inverting the different stomachs, an opening was made at the cardia, to prevent the inversion from injuring the internal parts; this opening was afterwards sewed up in such a manner as to be air-tight, and then the stomach was distended.

Where the parts are not represented of their natural size, the proportions in which they are reduced are marked upon the Plate.

IX. *Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir* Isaac Newton, *between two Object-glasses laid upon one another. By* William Herschel, *LL.D. F.R.S.*

Read February 5, 1807.

THE account given by Sir I. NEWTON, of the coloured arcs and rings which he discovered by laying two prisms or object-glasses upon each other, is highly interesting. He very justly remarks, that these phenomena are "of difficult consideration," but that "they may conduce to farther discoveries for "completing the theory of light, especially as to the consti-"tution of the parts of natural bodies on which their colours "or transparency depend."*

With regard to the explanation of the appearance of these coloured rings, which is given by Sir I. NEWTON, I must confess that it has never been satisfactory to me. He accounts for the production of the rings, by ascribing to the rays of light certain fits of easy reflection and easy transmission alternately returning and taking place with each ray at certain stated intervals.† But this, without mentioning particular objections, seems to be an hypothesis which cannot be easily reconciled with the minuteness and extreme velocity of the particles of which these rays, according to the NEWTONIAN theory, are composed.

* NEWTON's Optics, 4th ed. p. 169. † Ibid. p. 256.

The great beauty of the coloured rings, and the pleasing appearances arising from the different degrees of pressure of the two surfaces of the glasses against each other when they are formed, and especially the importance of the subject, have often excited my desire of enquiring farther into the cause of such interesting phenomena; and with a view to examine them properly I obtained, in the year 1792, the two object-glasses of HUYGENS, in the possession of the Royal Society, one of 122, the other of 170 feet focal length, and began a series of experiments with them, which, though many times interrupted by astronomical pursuits, has often been taken up again, and has lately been carried to a very considerable extent. The conclusions that may be drawn from them, though they may not perfectly account for all the phœnomena of the rings, are yet sufficiently well supported, and of such a nature as to point out several modifications of light that have been totally overlooked, and others that have never been properly discriminated. It will, therefore, be the aim of this paper to arrange and distinguish the various modifications of light in a clear and perspicuous order, and afterwards to give my sentiments upon the cause of the formation of the concentric rings. The avowed intricacy of the subject,* however, requires, in the first place, a minute detail of experiments, and afterwards a very gradual developement of the consequences to be deduced from them.

As the word modification will frequently be used, it may not be amiss to say, that when applied to light, it is intended to stand for a general expression of all the changes that are made in its colours, direction, or motion: thus, by the modifi-

* NEWTON'S Optics, 4th ed. p. 288; end of Obs. 12.

cation of reflection, light is thrown back; by that of refraction, it is bent from its former course; by the modification of dispersion, it is divided into colours, and so of the rest.

I. *Of different Methods to make one set of concentric Rings visible.*

In the beginning of my experiments I followed the NEWTONIAN example, and having laid the two object-glasses of HUYGENS upon one another I soon perceived the concentric rings. It is almost needless to say that I found all the NEWTONIAN observations of these rings completely verified; but as his experiments seemed to be too much confined for drawing general conclusions, I endeavoured to extend them: and by way of rendering the methods I point out very clear, I have given one easy particular instance of each, with the addition of a generalization of it, as follows:

First Method. On a table placed before a window I laid down a slip of glass the sides of which were perfectly plain, parallel, and highly polished. Upon this I laid a double convex lens of 26 inches focal length, and found that this arrangement gave me a set of beautiful concentric rings.

I viewed them with a double convex eye lens of $2\frac{1}{2}$ inches focus mounted upon an adjustable stand, by which simple apparatus I could examine them with great ease; and as it was not material to my present purpose by what obliquity of incidence of light I saw the rings, I received the rays from the window most conveniently when they fell upon the lens in an angle of about 30 degrees from the perpendicular, the eye being placed on the opposite side at an equal angle of elevation to receive the reflected rays.

Generalization. Instead of a plain slip of glass, the plain side of a plano-concave, or plano-convex lens of any focal length whatsoever may be used: and when the convex side of any lens is laid upon it, whatever may be the figure of the other surface, whether plain, concave, or convex, and whatever may be its focal length, a set of concentric rings will always be obtained. I have seen rings with lenses of all varieties of focus, from 170 feet down to one quarter of an inch. Even a common watch glass laid upon the same plain surface will give them.

To insure success, it is necessary that the glasses should be perfectly well cleaned from any adhering dust or soil, especially about the point of contact; and in laying them upon each other a little pressure should be used, accompanied at first with a little side motion, after which they must be left at rest.

If the surface of the incumbent lens, especially when it is of a very short focal length, is free from all imperfection and highly polished, the adjustment of the focus of the above mentioned eye-glass, which I always use for viewing the rings, is rather troublesome, in which case a small spot of ink made upon the lens will serve as an object for a sufficient adjustment to find the rings.

Second Method. Instead of the slip of glass, I laid down a well polished plain metalline mirror; and placing upon it the same 26-inch double convex lens, I saw again a complete set of concentric rings.

It is singular that, in this case, the rings reflected from a bright metalline surface will appear fainter than when the same lens is laid on a surface of glass reflecting but little light; this may however be accounted for by the brilliancy of the

metalline ground on which these faint rings are seen, the contrast of which will offuscate their feeble appearance.

Generalization. On the same metalline surface every variety of lenses may be laid, whatever be the figure of their upper surface, whether plain, concave, or convex, and whatever be their focal lengths, provided the lowest surface remains convex, and concentric rings will always be obtained; but for the reason mentioned in the preceding paragraph, very small lenses should not be used till the experimentalist has been familiarized with the method of seeing these rings, after which lenses of two inches focus, and gradually less, may be tried.

Third Method. Hitherto we have only used a plain surface upon which many sorts of glasses have been placed; in order therefore to obtain a still greater variety, I laid down a plano-convex lens of 15 inches focal length, and upon the convex surface of it I placed the 26-inch double convex lens, which produced a complete set of rings.

Fourth Method. The same lens placed upon a convex metalline mirror of about 15 inches focal length gave also a complete set of rings.

Generalization. These two cases admit of a much greater variety than the first and second methods; for here the incumbent glass may have not only one, but both its surfaces of any figure whatsoever; whether plain, concave, or convex; provided the radius of concavity, when concave lenses are laid upon the convex surface of glass or metal, is greater than that of the convexity on which they are laid.

The figure of the lowest surface of the subjacent substance, when it is glass, may also be plain, concave, or convex; and

the curvature of its upper surface, as well as of the mirror, may be such as to give them any focal length, provided the radius of their convexities is less than that of the concavity of an incumbent lens; in all which cases complete sets of concentric rings will be obtained.

Fifth Method. Into the concavity of a double concave glass of 8 inches focal length I placed a 7-inch double convex lens, and saw a very beautiful set of rings.

Sixth Method. Upon a 7 feet concave metalline mirror I placed the double convex 26-inch lens, and had a very fine set of rings.

Generalization. With these two last methods, whatever may be the radius of the concavity of the subjacent surface, provided it be greater than that of the convexity of the incumbent glass; and whatever may be the figure of the upper surface of the lenses that are placed upon the former, there will be produced concentric rings. The figure of the lowest surface of the subjacent glass may also be varied at pleasure, and still concentric rings will be obtained.

II. *Of seeing Rings by Transmission.*

The great variety of the different combinations of these differently figured glasses and mirrors will still admit of further addition, by using a different way of viewing the rings. Hitherto, the arrangement of the apparatus has been such as to make them visible only by reflection, which is evident, because all the experiments that have been pointed out may be made by the light of a candle placed so that the angle of incidence and of reflection towards the eye of the observer, may be equal. But Sir I. Newton has given us also an

observation where he saw these rings by transmission, in consequence of which I have again multiplied and varied the method of producing them that way, as follows:

First Method. On a slip of plain glass highly polished on both sides place the same double convex lens of 26 inches, which had already been used when the rings were seen by reflection. Take them both up together and hold them against the light of a window, in which position the concentric rings will be seen with great ease by transmitted light. But as the use of an eye-glass will not be convenient in this situation, it will be necessary to put on a pair of spectacles with glasses of 5, 6, or 7 inches focus, to magnify the rings in order to see them more readily.

Second Method. It would be easy to construct an apparatus for viewing the rings by transmission fitted with a proper eye-glass; but other methods of effecting the same purpose are preferable. Thus, if the two glasses that are to give the rings be laid upon a hollow stand, a candle placed at a proper angle and distance under them will show the rings conveniently by transmitted light, while the observer and the apparatus remain in the same situation as if they were to be seen by reflection.

Third Method. A still more eligible way is to use daylight received upon a plain metalline mirror reflecting it upwards to the glasses placed over it, as practised in the construction of the common double microscope; but I forbear entering into a farther detail of this last and most useful way of seeing rings by transmission, as I shall soon have occasion to say more on the same subject.

Generalization. Every combination of glasses that has been explained in the first, third, and fifth methods of seeing rings

by reflection will also give them by transmission, when exposed to the light in any of the three ways that have now been pointed out. When these are added to the former, it will be allowed that we have an extensive variety of arrangements for every desirable purpose of making experiments upon rings, as far as single sets of them are concerned.

III. *Of Shadows.*

When two or more sets of rings are to be seen, it will require some artificial means, not only to examine them critically, but even to perceive them; and here the shadow of some slender opaque body will be of eminent service. To cast shadows of a proper size and upon places where they are wanted, a pointed penknife may be used as follows.

When a plain slip of glass or convex lens is laid down, and the point of a penknife is brought over either of them, it will cast two shadows, one of which may be seen on the first surface of the glass or lens, and the other on the lowest.

When two slips of glass are laid upon each other, or a convex lens upon one slip, so that both are in contact, the penknife will give three shadows; but if the convex lens should be of a very short focus, or the slips of glass a little separated, four of them may be perceived; for in that case there will be one formed on the lowest surface of the incumbent glass or lens; but in my distinction of shadows this will not be noticed. Of the three shadows thus formed the second will be darker than the first, but the third will be faint. When a piece of looking glass is substituted for the lowest slip the third shadow will be the strongest.

Three slips of glass in contact, or two slips with a lens

upon them, or also a looking glass, a slip and a lens put together, will give four shadows, one from each upper surface and one from the bottom of the lowest of them.

In all these cases a metalline mirror may be laid under the same arrangement without adding to the number of shadows, its effect being only to render them more intense and distinct.

The shadows may be distinguished by the following method. When the point of the penknife is made to touch the surface of the uppermost glass or lens, it will touch the point of its own shadow, which may thus at any time be easily ascertained: and this in all cases I call the first shadow; that which is next to it, the second; after which follows the third, and so on.

In receding from the point, the shadows will mix together, and thus become more intense; but which, or how many of them are united together, may always be known by the points of the shadows.

When a shadow is to be thrown upon any required place, hold the penknife nearly half an inch above the glasses, and advance its edge foremost gradually towards the incident light. The front should be held a little downwards to keep the light from the underside of the penknife, and the shadows to be used should be obtained from a narrow part of it.

With this preparatory information it will be easy to point out the use that is to be made of the shadows when they are wanted.

IV. *Of two sets of Rings.*

I shall now proceed to describe a somewhat more complicated way of observation, by which two complete sets of concentric rings may be seen at once. The new or additional set

will furnish us with an opportunity of examining rings in situations where they have never been seen before, which will be of eminent service for investigating the cause of their origin, and with the assistance of the shadows to be formed, as has been explained, we shall not find it difficult to see them in these situations.

First Method. Upon a well polished piece of good looking-glass lay down a double convex lens of about 20 inches focus. When the eye glass has been adjusted as usual for seeing one set of rings, make the shadow of the penknife in the order which has been described, pass over the lens; then, as it sometimes happens in this arrangement that no rings are easily to be seen, the shadow will, in its passage over the surface, show where they are situated. When a set of them is perceived, which is generally the primary one, bring the third shadow of the penknife over it, in which situation it will be seen to the greatest advantage.

Then, if at the same time a secondary set of rings has not yet been discovered, it will certainly be perceived when the second shadow of the penknife is brought upon the primary set. As soon as it has been found out, the compound shadow, consisting of all the three shadows united, may then be thrown upon this secondary set, in order to view it at leisure and in perfection. But this compound shadow should be taken no farther from the point than is necessary to cover it; nor should the third shadow touch the primary set. The two sets are so near together, that many of the rings of one set intersect some of the other.

When a sight of the secondary set has been once obtained, it will be very easy to view it alternately with the primary one

by a slight motion of the penknife, so as to make the third shadow of it go from one set to the other.

Besides the use of the shadows, there is another way to make rings visible when they cannot be easily perceived, which is to take hold of the lens with both hands, to press it alternately a little more with one than with the other; a tilting motion, given to the lens in this manner, will move the two sets of rings from side to side; and as it is well known that a faint object in motion may be sooner perceived than when it is at rest, both sets of rings will by these means be generally detected together.

It will also contribute much to facilitate the method of seeing two sets of rings, if we receive the light in a more oblique angle of incidence, such as 40, 50, or even 60 degrees. This will increase the distance between the centers of the primary and secondary sets, and at the same time occasion a more copious reflection of light.

Instead of a common looking-glass a convex glass mirror may be used, on which may be placed either a plain, a concave, or a convex surface of any lens or glass, and two sets of rings will be obtained.

In the same manner, by laying upon a concave glass mirror a convex lens, we shall also have two sets of rings.

The generalizations that have been mentioned when one set of rings was proposed to be obtained, may be easily applied with proper regulations, according to the circumstances of the case, not only to the method by glass mirrors already mentioned, but likewise to all those that follow hereafter, and need not be particularized for the future. In the choice of the surfaces to be joined, we have only to select such as will form a

central contact, the focal length of the lenses and the figure of the upper surface being variable at pleasure.

Second Method. On a plain metalline mirror I laid a parallel slip of glass, and placed upon it the convex surface of a 17-inch plano-convex lens, by which means two sets of rings were produced.

Upon the same mirror the plain side of the plano-convex glass may be laid instead of the plain slip, and any plain, convex, or concave surface being placed upon the convexity of the subjacent lens, will give two sets of rings.

The plain side of a plano-concave glass may also be placed upon the same mirror, and into the concavity may be laid any lens that will make a central contact with it, by which arrangement two sets of rings will be obtained.

Third Method. Upon a small well polished slip of glass place another slip of the same size, and upon them lay a 39-inch double convex lens. This will produce two sets of rings; one of them reflected from the upper surface of the first slip of glass, and the other from that of the second.

Instead of the uppermost plain slip of glass we may place upon the lowest slip the plain side of a plano-convex or plano-concave lens, and the same variety which has been explained in the third method, by using any incumbent lens that will make a central contact, either with the convexity or concavity of the subjacent glass, will always produce two sets of rings.

Fourth Method. A more refined but rather more difficult way of seeing two sets of rings, is to lay a plain slip of glass on a piece of black paper, and when a convex lens is placed upon the slip, there may be perceived, but not without

particular attention, not only the first set, which has already been pointed out as reflected from the first surface of the slip, but also a faint secondary set from the lowest surface of the same slip of glass.

It will be less difficult to see two sets of rings by a reflection from both surfaces of the same glass, if we use, for instance, a double concave of 8 inches focus with a double convex of $7\frac{1}{2}$ inches placed upon it. For, as it is well known that glass will reflect more light from the farthest surface when air rather than a denser medium is in contact with it, the hollow space of the 8-inch concave will give a pretty strong reflection of the secondary set.

Fifth Method. The use that is intended to be made of two sets of rings requires that one of them should be dependent upon the other: this is a circumstance that will be explained hereafter, but the following instance, where two independent sets of rings are given, will partly anticipate the subject. When a double convex lens of 50 inches is laid down with a slip of glass placed upon it, and another double convex one of 26 inches is then placed upon the slip, we get two sets of rings of different sizes; the large rings are from the 50-inch glass, the small rings from the 26-inch one. They are to be seen with great ease, because they are each of them primary. By tilting the incumbent lens or the slip of glass these two sets of rings may be made to cross each other in any direction; the small set may be laid upon the large one, or either of them may be separately removed towards any part of the glass. This will be sufficient to show that they have no connection with each other. The phænomena of the motions, and of the various colours and sizes assumed by these rings,

when different pressures and tiltings of the glasses are used will afford some entertainment. With the assistance of the shadow of the penknife the secondary set belonging to the rings from the 26-inch lens will be added to the other two sets; but in tilting the glasses this set will never leave its primary one, while that from the 50-inch lens may be made to go any where across the other two.

V. *Of three Sets of Rings.*

To see three sets of concentric rings at once is attended with some difficulty, but by the assistance of the methods of tilting the glasses and making use of the multiplied shadows of a penknife we may see them very well, when there is a sufficient illumination of bright daylight.

First Method. A 26-inch double convex lens placed upon three slips of plain glass will give three sets of rings. The slips of glass should be nearly 2-tenths of an inch thick, otherwise the different sets will not be sufficiently separated. When all the glasses are in full contact the first and second sets may be seen with a little pressure and a small motion, and, if circumstances are favourable the third, which is the faintest, will also appear. If it cannot be seen, some of the compound shadows of the penknife must be thrown upon it; for in this case there will be five shadows visible, several of which will fall together and give different intensity to their mixture.

Second Method. When a single slip of glass, with a 34-inch lens upon it, is placed upon a piece of good looking-glass, three sets of rings may be seen: the first and third sets are pretty bright, and will be perceived by only pressing the lens a little upon the slip of glass; after which it will be easy to find the

second set with the assistance of the proper shadow. In this case four shadows will be seen; and when the third shadow is upon the first set, the fourth will be over the second set and render it visible.

Third Method. When two slips of glass are laid upon a plain metalline mirror, then a 26-inch lens placed upon the slips will produce three sets of rings; but it is not very easy to perceive them. By a tilting motion the third set will generally appear like a small white circle, which at a proper distance will follow the movement of the first set. As soon as the first and third sets are in view the third shadow of the penknife may be brought over the first set, by which means the fourth shadow will come upon the second set, and in this position of the apparatus it will become visible.

Fourth Method. On a plain metalline mirror lay one slip of glass, but with a small piece of wood at one end under it, so that it may be kept about one-tenth of an inch from the mirror, and form an inclined plane. A 26-inch lens laid upon the slip of glass will give three sets of rings. Two of them will easily be seen; and when the shadow of the penknife is held between them the third set will also be perceived. There is but one shadow visible in this arrangement, which is the third; the first and second shadows being lost in the bright reflection from the mirror.

Fifth Method. I placed a $6\frac{3}{4}$-inch double convex upon an 8-inch double concave, and laid both together upon a plain slip of glass. This arrangement gave three sets of rings. They may be seen without the assistance of shadows, by using only pressure and tilting. The first had a black and the other two had white centers.

VI. *Of four Sets of Rings.*

The difficulty of seeing many sets of rings increases with their number, yet by a proper attention to the directions that are given four sets of concentric rings may be seen.

First Method. Let a slip of glass, with a 26-inch lens laid upon it, be placed upon a piece of looking-glass. Under one end of the slip, a small piece of wood one-tenth of an inch thick must be put to keep it from touching the looking-glass. This arrangement will give us four sets of rings. The first, third, and fourth may easily be seen, but the second set will require some management. Of the three shadows, which this apparatus gives, the second and third must be brought between the first and fourth sets of rings, in which situation the second set of rings will become visible.

Second Method. When three slips of glass are laid upon a metalline mirror, and a plano-convex lens of about 17 inches focus is placed with its convex side upon them, four sets of rings may be seen; but this experiment requires a very bright day, and very clean, highly polished slips of plain glass. Nor can it be successful unless all the foregoing methods of seeing multiplied sets of rings are become familiar and easy.

I have seen occasionally, not only four and five, but even six sets of concentric rings, from a very simple arrangement of glasses: they arise from reiterated internal reflections; but it will not be necessary to carry this account of seeing multiplied sets of rings to a greater length.

VII. *Of the Size of the Rings.*

The diameter of the concentric rings depends upon the

radius of the curvature of the surfaces between which they are formed. Curvatures of a short radius, *cæteris paribus*, give smaller rings than those of a longer; but Sir I. NEWTON having already treated on this part of the subject at large, it will not be necessary to enter farther into it.

I should however remark, that when two curves are concerned, it is the application of them to each other that will determine the size of the rings, so that large ones may be produced from curvatures of a very short radius. A double convex lens of $2\frac{1}{4}$-inches focus, for instance, when it is laid upon a double concave which is but little more in focal length, gives rings that are larger than those from a lens of 26 inches laid upon a plain slip of glass.

VIII. *Of Contact.*

The size of the rings is considerably affected by pressure. They grow larger when the two surfaces that form them are pressed closer together, and diminish when the pressure is gradually removed. The smallest ring of a set may be increased by this means to double and treble its former diameter; but as the common or natural pressure of glasses laid upon any flat or curved surface is occasioned by their weight, the variations of pressure will not be very considerable when they are left to assume their own distance or contact. To produce that situation, however, which is generally called contact, it will always be necessary to give a little motion backwards and forwards to the incumbent lens or glass, accompanied with some moderate pressure, after which it may be left to settle properly by its own weight.

IX. *Of measuring Rings.*

It may be supposed from what has been said concerning the kind of contact, which is required for glasses to produce rings, that an attempt to take absolute measures must be liable to great inaccuracy. This was fully proved to me when I wanted to ascertain, in the year 1792, whether a lens laid upon a metalline surface would give rings of an equal diameter with those it gave when placed on glass. The measures differed so much that I was at first deceived; but on proper consideration it appeared that the HUYGENIAN object glass, of 122 feet focus, which I used for the experiment, could not so easily be brought to the same contact on metal as on glass; nor can we ever be well assured that an equal distance between the two surfaces in both cases has been actually obtained. The colour of the central point, as will be shown hereafter, may serve as a direction; but even that cannot be easily made equal in both cases. By taking a sufficient number of measures of any given ring of a set, when a glass of a sufficient focal length is used, we may however determine its diameter to about the 25th or 30th part of its dimension.

Relative measures, for ascertaining the proportion of the different rings in the same set to each other, may be more accurately taken, for in that case the contact with them all will remain the same, if we do not disturb the glasses during the time of measuring.

X. *Of the Number of Rings.*

When there is a sufficient illumination, many concentric rings in every set will be perceived; in the primary set we see

generally 8, 9, or 10, very conveniently. By holding the eye in the most favourable situation I have often counted near 20, and the number of them is generally lost when they grow too narrow and minute to be perceived, so that we can never be said fairly to have counted them to their full extent. In the second set I have seen as many as in the first, and they are full as bright. The third set, when it is seen by a metalline mirror under two slips, will be brighter than the second, and almost as bright as the first: I have easily counted 7, 8, and 9 rings.

XI. *Of the Effect of Pressure on the Colour of the Rings.*

When a double convex object glass of 14 or 15 feet focus is laid on a plain slip of glass, the first colours that make their faintest appearance will be red surrounded by green; the smallest pressure will turn the center into green surrounded by red: an additional pressure will give a red center again, and so on till there have been so many successive alterations as to give us six or seven times a red center, after which the greatest pressure will only produce a very large black one surrounded by white.

When the rings are seen by transmission, the colours are in the same manner subject to a gradual alternate change occasioned by pressure; but when that is carried to its full extent, the center of the rings will be a large white spot surrounded by black.

The succession and addition of the other prismatic colours after the first or second change, in both cases is extremely beautiful; but as the experiment may be so easily made, a

description, which certainly would fall short of an actual view of these phenomena, will not be necessary.

When the rings are produced by curves of a very short radius, and the incumbent lens is in full contact with the slip of glass, they will be alternately black and white; but by lessening the contact, I have seen, even with a double convex lens of no more than two-tenths of an inch focus, the center of the rings white, red, green, yellow, and black, at pleasure. In this case I used an eye-glass of one inch focus; but as it requires much practice to manage such small glasses, the experiment may be more conveniently made by placing a double convex lens of 2-inch focus on a plain slip of glass, and viewing the rings by an eye glass of $2\frac{1}{2}$ inches; then having first brought the lens into full contact, the rings will be only black and white, but by gently lifting up or tilting the lens, the center of the rings will assume various colours at pleasure.

XII. *Of diluting and concentrating the Colours.*

Lifting up or tilting a lens being subject to great uncertainty, a surer way of acting upon the colours of the rings is by dilution and concentration. After having seen that very small lenses give only black and white when in full contact, we may gradually take others of a longer focus. With a double convex lens of four inches the outward rings will begin to assume a faint red colour. With 5, 6, and 7, this appearance will increase; and proceeding with lenses of a larger focus, when we come to about 16, 18, or 20 inches, green rings will gradually make their appearance.

This and other colours come on much sooner if the center

of the lens is not kept in a black contact, which, in these experiments must be attended to.

A lens of 26 inches not only shows black, white, red, and green rings, but the central black begins already to be diluted so as to incline to violet, indigo, or blue. With one of 34, the white about the dark center begins to be diluted, and shows a kind of gray inclining to yellow. With 42 and 48, yellow rings begin to become visible. With 55 and 59, blue rings show themselves very plainly. With a focal length of 9 and 11 feet, orange many be distinguished from the yellow, and indigo from the blue. With 14 feet, some violet becomes visible. When the 122 feet HUYGENIAN glass is laid on a plain slip, and well settled upon it, the central colour is then sufficiently diluted to show that the dark spot, which in small lenses, when concentrated, had the appearance of black, is now drawn out into violet, indigo, and blue, with little admixture of green; and that the white ring, which used to be about the central spot, is turned partly green with a surrounding yellow, orange, and red-coloured space or ring; by which means we seem to have a fair analysis of our former compound black and white center.

One of my slips of glass, which is probably a little concave, gave the rings still larger when the 122 feet glass was firmly pressed against it. I used a little side motion at the same time, and brought the glasses into such contact that they adhered sufficiently to be lifted up together. With this adhesion I perceived a colour surrounding a dark center which I have never seen in any prismatic spectrum. It is a kind of light brown, resembling the colour of a certain sort of Spanish snuff. The 170 feet object glass showed the same colour also very clearly.

XIII. *Of the Order of the Colours.*

The arrangement of the colours in each compound ring or alternation, seen by reflection is, that the most refrangible rays are nearest the center; and the same order takes place when seen by transmission. We have already shown that when a full dilution of the colours was obtained their arrangement was violet, indigo, blue, green, yellow, orange, and red; and the same order will hold good when the colours are gradually concentrated again; for though some of them should vanish before others, those that remain will always be found to agree with the same arrangement.

If the rings should chance to be red and green alternately, a doubt might arise which of them is nearest the center; but by the method of dilution, a little pressure, or some small increase of the focal length of the incumbent lens, there will be introduced an orange tint between them, which will immediately ascertain the order of the colours.

In the second set of rings the same order is still preserved as in the first; and the same arrangement takes place in the third set as well as in the fourth. In all of them the most refrangible rays produce the smallest rings.

XIV. *Of the alternate Colour and Size of the Rings belonging to the primary and dependent Sets.*

When two sets of rings are seen at once, and the colour of the center of the primary set is black, that of the secondary will be white; if the former is white, the latter will be black. The same alternation will take place if the colour of the center of the primary set should be red or orange; for then the

center of the secondary one will be green; or if the former happens to be green, the latter will be red or orange. At the same time there will be a similar alternation in the size of rings; for the white rings in one set will be of the diameter of the black in the other; or the orange rings of the former will be of equal magnitude with the green of the latter.

When three sets of rings are to be seen, the second and third sets will be alike in colour and size, but alternate in both particulars with the primary set.

The same thing will happen when four sets are visible; for all the sets that are formed from the primary one will resemble each other, but will be alternate in the colour and dimensions of their rings with those of the primary set.

XV. *Of the sudden Change of the Size and Colour of the Rings in different Sets.*

When two sets of rings are viewed which are dependent upon each other, the colour of their centers and of all the rings in each set, may be made to undergo a sudden change by the approach of the shadow of the point of a penknife or other opaque slender body. To view this phenomenon properly, let a 16-inch double convex lens be laid upon a piece of looking-glass, and when the contact between them has been made to give the primary set with a black center, that of the secondary will be white. To keep the lens in this contact, a pretty heavy plate of lead with a circular hole in it of nearly the diameter of the lens should be laid upon it. The margin of the hole must be tapering, that no obstruction may be made to either the incident or reflected light. When this is properly arranged, bring the third shadow of the penknife upon the

primary set, which is that towards the light. The real colours of this and the secondary set will then be seen to the greatest advantage. When the third shadow is advanced till it covers the second set, the second shadow will at the same time fall upon the first set, and the colour of the centers, and of all the rings in both sets, will undergo a sudden transformation from black to white and white to black.

The alternation of the colour is accompanied with a change of size, for as the white rings before the change were of a different diameter from the black ones, these latter, having now assumed a black colour, will be of a different size from the former black ones.

When the weight is taken from the lens the black contact will be changed into some other. In the present experiment it happened that the primary set got an orange coloured center and the secondary a green one. The same way of proceeding with the direction of the shadow being then pursued, the orange center was instantly changed to a green one, while at the same moment the green center was turned into orange. With a different contact I have had the primary set with a blue center and the secondary with a deep yellow one; and by bringing the second and third shadows alternately over the primary set, the blue center was changed to a yellow, and the yellow center to a blue one; and all the rings of both sets had their share in the transformation of colour and size.

If there are three sets of rings, and the primary set has a black center, the other two will have a white one; and when the lowest shadow is made to fall on the third set, the central colour of all the three sets will be suddenly changed, the first from black to white, the other two from white to black.

A full explanation of these changes, which at first sight

have the appearance of a magical delusion, will be found in a future article.

XVI. *Of the Course of the Rays by which different Sets of Rings are seen.*

In order to determine the course of the rays, which give the rings both by reflection and by transmission, we should begin from the place whence the light proceeds that forms them. In figure 1, we have a plano-convex lens laid upon three slips of glass, under which a metalline mirror is placed. An incident ray, 1, 2, is transmitted through the first and second surface of the lens, and comes to the point of contact at 3. Here the rings are formed, and are both reflected and transmitted: they are reflected from the upper surface of the first slip, and pass from 3 to the eye at 4: they are also transmitted through the first slip of glass from 3 to 5; and at 5 they are again both reflected and transmitted; reflected from 5 to 6, and transmitted from 5 to 7; from 7 they are reflected to 8, and transmitted to 9; and lastly they are reflected from 9 to 10. And thus four complete sets of rings will be seen at 4, 6, 8, and 10.

The most convenient way of viewing the same rings by transmission, is that which has been mentioned in the second article of this paper, when light is conveyed upwards by reflection. In figure 2, consisting of the same arrangement of glasses as before, the light by which the rings are to be seen comes either from 1, 2, or 3, or from all these places together, and being reflected at 4, 5, and 6, rises up by transmission to the point of contact at 7, where the rings are formed. Here they are both transmitted up to the eye at 8, and reflected down to 9; from 9 they are reflected up to 10 and transmitted down to 11; from 11 they are reflected to 12 and transmitted

to 13; and lastly, from 13 they are reflected to 14; so, that again four sets of rings will be seen at 8, 10, 12, and 14.

This being a theoretical way of conceiving how the rays of light may produce the effects, it will be required to show by experiments that this is the actual progress of the rays, and that all the sets of rings we perceive are really reflected or transmitted in the manner that has been pointed out; but as we have so many reflections and transmissions before us, it will be necessary to confine these expressions to one particular signification when they are applied to a set of rings.

When the center of the rings is seen at the point of contact, it is a primary set; and I call it reflected, when the rays which come to that point and form the rings undergo an immediate reflection. But I call it transmitted, when the rays after having formed the rings about the point of contact are immediately transmitted.

Thus in figure 3 and 4 the rays *a b c*, *d e f*, give reflected sets of rings; and the rays *g h i*, *k l m*, in figure 5 and 6, give transmitted sets.

In this denomination, no account is taken of the course of the rays before they come to *a*, *d*, *g*, *k*; nor of what becomes of them after their arrival at *c*, *f*, *i*, *m*: they may either come to those places or go from them by one or more transmissions or reflections, as the case may require; but our denomination will relate only to their course immediately after the formation of the rings between the glasses.

The secondary and other dependent sets will also be called reflected or transmitted by the same definition: and as a set of these rings formed originally by reflection may come to the eye by one or more subsequent transmissions; or being

formed by transmission, may at last be seen by a reflection from some interposed surface, these subsequent transmissions or reflections are to be regarded only as convenient ways to get a good sight of them.

With this definition in view, and with the assistance of a principle which has already been proved by experiments, we may explain some very intricate phenomena; and the satisfactory manner of accounting for them will establish the truth of the theory relating to the course of rays that has been described.

The principle to which I refer is, that when the pressure is such as to give a black center to a set of rings seen by reflection, the center of the same set, with the same pressure of the glasses seen by transmission will be white.*

I have only mentioned black and white, but any other alternate colours, which the rings or centers of the two sets may assume, are included in the same predicament.

XVII. *Why two connected Sets of Rings are of alternate Colours.*

It has already been shown, when two sets of rings are seen, that their colours are alternate, and that the approach of the shadow of a penknife will cause a sudden change of them to take place. I shall now prove that this is a very obvious consequence of the course of rays that has been proposed. Let figure 7 and 8 represent the arrangement given in a preceding article, where a 16-inch lens was laid upon a looking-glass, and gave two sets of rings with centers of different colours: but let figure 7 give them by one set of rays, and figure 8 by another. Then, if the incident rays come in the direction which

* See Article XI. of this Paper.

is represented in figure 7, it is evident that we see the primary set with its center at 2 by reflection, and the secondary one at 4 by transmission. Hence it follows, in consequence of the admitted principle, that if the contact is such as to give us the primary set with a black center, the secondary set must have a white one; and thus the reason of the alternation is explained.

But if the rays come as represented in figure 8, we see the primary set by transmission, and the secondary one by reflection; therefore, with an equal pressure of the glasses, the primary center must now be white, and the secondary one black.

Without being well acquainted with this double course of rays, we shall be liable to frequent mistakes in our estimation of the colour of the centers of two sets of rings; for by a certain position of the light, or of the eye, we may see one set by one light and the other set by the other.

XVIII. *Of the Cause of the sudden Change of the Colours.*

Having thus accounted for the alternation of the central colours, we may easily conceive that the interposition of the penknife must have an instantaneous effect upon them. When it stops the rays of figure 7, which will happen when its second shadow falls upon the primary set, the rings will then be seen by the rays 1, 2, 3, 4, and 1, 2, 3, 5, 6, of figure 8. When it stops the rays of figure 8, which must happen when the third shadow falls upon the primary set, we then see both sets by the rays 1, 2, 3, and 1, 2, 4, 5, of figure 7. When the penknife is quite removed both sets of rays will come to the point of contact, and in some respects interfere with each

other; but the strongest of the two, which is generally the direct light of figure 7, will prevail. This affords a complete explanation of all the observed phenomena: by the the rays of figure 7 the centers will be black and white; by those of figure 8 they will be white and black; and by both we shall not see the first set so well as when the third shadow being upon it, has taken away the rays of figure 8: indeed we can hardly see the secondary set at all, till the shadow of the penknife has covered either the rays of figure 7 or of figure 8.

As soon as we are a little practised in the management of the rays, by knowing their course, we may change the colour so gradually as to have half the center white while the other half shall still remain black; and the same may be done with green and orange, or blue and yellow centers. The rings of both sets will also participate in the gradual change; and thus what has been said of the course of rays in the 16th article will again be confirmed.

XIX. *Of the Place where the different Sets of Rings are to be seen.*

By an application of the same course of the rays, we may now also determine the situation of the place where the different sets of rings are seen: for according to what has been said in the foregoing article, the situation of the primary set should be between the lens and the surface of the looking-glass: and the place of the secondary one at the metalline coating of the lowest surface. To try whether this be actually as represented, let us substitute a metalline mirror with a slip of glass laid upon it in the room of the piece of looking-glass; and let there be interposed a short bit of wood, one-tenth of

an inch thick, between the slip of glass and the mirror, so as to keep up that end of the slip which is towards the light. This arrangement is represented in figure 9, where both sets of rays are delineated. Then if we interpose a narrow tapering strip of card, discoloured with japan ink, between the slip of glass and the mirror, so as to cover it at 7, we do not only still perceive the primary set, but see it better than before: which proves that being situated above the slip of glass the card below cannot cover it. If on the contrary we insert the strip of card far enough, that it may at the same time cover the mirror both at 4 and at 7, we shall lose the secondary set, which proves that its situation was on the face of the mirror.

When several sets of rings are to be perceived by the same eye-glass, and they are placed at different distances, a particular adjustment of it will be required for each set, in order to see it well defined. This will be very sensible when we attempt to see three or four sets, each of them situated lower than the preceding; for without a previous adjustment to the distance of the set intended to be viewed we shall be seldom successful; and this is therefore a corroborating proof of the situation that has been assigned to different sets of rings.

XX. *Of the Connection between different Sets of Rings.*

It will now be easy to explain in what manner different sets of rings are connected, and why they have been called primary and dependent. When the incident rays come to the point of contact and form a set of rings, I call it the primary one: when this is formed some of the same rays are continued by transmission or reflection, but modified so as to convey an image of the primary set with opposite colours forward

through any number of successive transmissions or reflections; whenever this image comes to the eye, a set of rings will again be seen, which is a dependent one. Many proofs of the dependency of second, third, and fourth sets of rings upon their primary one may be given; I shall only mention a few.

When two sets of rings are seen by a lens placed upon a looking-glass, the center of the secondary set will always remain in the same plane with the incident and reflected rays passing through the center of the primary one. If the point of contact, by tilting is changed, the secondary set will follow the motion of the primary set; and if the looking-glass is turned about, the secondary will be made to describe a circle upon that part of the looking-glass which surrounds the primary one as a center. If there is a defect in the center or in the rings of the primary set there will be exactly the same defect in the secondary one; and if the rays that cause the primary set are eclipsed, both sets will be lost together. If the colour of the primary one is changed, that of the secondary will also undergo its alternate change, and the same thing will hold good of all the dependent rings when three or four sets of them are seen that have the same primary one.

The dependency of all the sets on their primary one may also be perceived when we change the obliquity of the incident light; for the centers of the rings will recede from one another when that is increased and draw together when we lessen it, which may go so far that by an incidence nearly perpendicular we shall bring the dependent sets of rings almost under the primary one.

XXI. *To account for the Appearance of several Sets of Rings with the same coloured Centers.*

It has often happened that the colour of the centers of different sets was not what the theory of the alternation of the central colours would have induced me to expect: I have seen two, three, and even four sets of rings, all of which had a white center. We are however now sufficiently prepared to account for every appearance relating to the colour of rings and their centers.

Let an arrangement of glasses be as in figure 9. When this is laid down so as to receive an illumination of day light, which should not be strong, nor should it be very oblique, the reflection from the mirror will then exceed that from the surface of glass; therefore the primary set will be seen by the rays 6, 7, coming to the mirror at 7, and going through the point of contact in the direction 7, 2, 3, which proves it to be a set that is seen by transmission, and it will therefore have a white center. The rays 1, 2, 4, passing through the point of contact, will also form a transmitted set with a white center, which will be seen when the reflection from 4 to 5 conveys it to the eye. But these two sets have no connection with each other; and as primary sets are independent of all other sets, I have only to prove that this secondary set belongs not to the primary one which is seen, but to another invisible one. This may be done as follows.

Introduce the black strip of card that has been mentioned before, till it covers the mirror at 7; this will take away the strong reflection of light which overpowers the feeble illumination of the rays 1, 2, 3; and the real hitherto eclipsed pri-

mary set belonging to the secondary one with a white center, will instantly make its appearance with a black one. We may alternately withdraw and introduce again the strip of card, and the center of the primary set will be as often changed from one colour to its opposite; but the secondary set, not being dependent on the rays 6, 7, will not be in the least affected by the change.

If the contact should have been such as to give both sets with orange centers, the introduction of the strip of card will prove that the set which is primary to the other has really a green center.

Another way of destroying the illusion is to expose the same arrangement to a brighter light, and at the same time to increase the obliquity of the angle of incidence; this will give a sufficient reflection from the surface of the glass to be no longer subject to the former deceptive appearance; for now the center of the primary set will be black, as it ought to be.

XXII. *Of the reflecting Surfaces.*

The rays of light that form rings between glasses, must undergo certain modifications by some of the surfaces through which they pass, or from which they are reflected; and to find out the nature of these modifications, it will be necessary to examine which surfaces are efficient. As we see rings by reflection and also by transmission, I shall begin with the most simple, and show experimentally the situation of the surface that reflects, not only the primary, but also the secondary sets of rings.

Upon a slip of glass, the lowest surface of which was deprived of its polish by emery, I laid an object-glass of 21 feet

focal length, and saw a very complete set of rings. I then put the same glass upon a plain metalline mirror, and saw likewise a set of them. They were consequently not reflected from the lowest surface of the subjacent glass or metal.

It will easily be understood, that were we to lay the same object glass upon a slip of glass emeried on both sides, or upon an unpolished metal, no rings would be seen. It is therefore neither from the first surface of the incumbent object-glass, nor from its lowest, that they are reflected; for if they could be formed without the modification of reflection from the upper surface of a subjacent glass or metal, they would still be seen when laid on rough surfaces; and consequently, the efficient reflecting surface, by which we see primary sets of rings, is that which is immediately under the point of contact.

To see a secondary set of rings by reflection, is only an inversion of the method of seeing a primary one. For instance, when a lens is laid upon a looking-glass, the course of the rays represented in figure 8, will show that the rays 1, 2, 3, 5, 6, by which a secondary set is seen, are reflected about the point of contact at 3, and that the lowest surface of the incumbent lens is therefore the efficient reflecting one; and thus it is proved, that in either case of seeing reflected rings, one of the surfaces that are joined at the point of contact contributes to their formation by a certain modification of reflection.

XXIII. *Of the transmitting Surfaces*

It would seem to be almost self-evident, that when a set of rings is seen by transmission, the light which occasions them must come through all the four surfaces of the two glasses which are employed; and yet it may be shown that this is not

necessary. We may, for instance, convey light into the body of the subjacent glass through its first surface, and let it be reflected within the glass at a proper angle, so that it may come up through the point of contact, and reach the eye, having been transmitted through no more than three surfaces. To prove this I used a small box, blackened on the inside, and covered with a piece of black pasteboard, which had a hole of about half an inch in the middle. Over this hole I laid a slip of glass with a 56-inch lens upon it; and viewed a set of rings given by this arrangement very obliquely, that the reflection from the slip of glass might be copious. Then guarding the point of contact between the lens and the slip of glass from the direct incident light, I saw the rings, after the colour of their center had been changed, by means of an internal reflection from the lowest surface of the slip of glass; by which it rose up through the point of contact, and formed the primary set of rings, without having been transmitted through the lowest surface of the subjacent glass. The number of transmitted surfaces is therefore by this experiment reduced to three; but I shall soon have an opportunity of showing that so many are not required for the purpose of forming the rings.

XXIV. *Of the Action of the first Surface.*

We have already shown that two sets of rings may be seen by using a lens laid upon a slip of glass; in which case, therefore, whether we see the rings by reflection or by transmission, no more than four surfaces can be essential to their formation. In the following experiments for investigating the action of these surfaces I have preferred metalline reflection, when glass was not required, that the apparatus might be more simple.

Upon a plain metalline mirror I laid a double convex lens, having a strong emery scratch on its uper surface. When I saw the rings through the scratch, they appeared to have a black mark across them. By tilting the lens, I brought the center of the rings upon the projection of the scratch, so that the incident light was obliged to come through the scratch to the rings, and the black mark was again visible upon them, but much stronger than before. In neither of the situations were the rings disfigured. The stronger mark was owing to the interception of the incident light, but when the rings had received their full illumination the mark was weaker, because in the latter case the rings themselves were probably complete, but in the former deficient.

I placed a lens that had a very scabrous polish on one side, but was highly polished on the other, upon a metalline mirror. The defective side being uppermost, I did not find that its scabrousness had any distorting effect upon the rings.

I splintered off the edge of a plain slip of glass; it broke as it usually does with a waving striated, curved slope coming to an edge. The splintered part was placed upon a convex metalline mirror of 2-inch focus, as in figure 10. The irregularity of the striated surface through which the incident ray 1, 2, was made to pass had very little effect upon the form of the rings; the striæ appearing only like fine dark lines, with hardly any visible distortion; but when, by tilting the returning ray, 2, 3, was also brought over the striated surface, the rings were much disfigured. This experiment therefore seems to prove that a very regular refraction of light by the first surface is not necessary; for though the rings were much disfigured when the returning light came through the

splintered defect, this is no more than what must happen to the appearance of every object which is seen through a distorting medium.

I laid the convex side of a plano-convex lens of 2,8-inch focus with a diameter of 1,5 upon a plain mirror, and when I saw a set of rings I tilted the lens so as to bring the point of contact to the very edge of the lens, both towards the light and from the light, which, on account of the large diameter of the lens, gave a great variety in the angle of incidence to the rays which formed the rings; but no difference in their size or appearance could be perceived. This seems to prove that no modification of the first surface in which the angle of incidence is concerned, such as refraction and dispersion, has any share in the production of the rings, and that it acts merely by the intromission of light; and though even this is not without being influenced by a change of the angle, it can only produce a small difference in the brightness of the rings.

A more forcible argument, that leads to the same conclusion, is as follows. Laying down three 54-inch double convex lenses, I placed upon the first the plain side of a plano-convex lens of $\frac{5}{8}$ inch focus; upon the second, a plain slip of glass; and upon the third, the plain side of a plano-concave lens also $\frac{5}{8}$ inch focus. I had before tried the same experiment with glasses of a greater focal length, but selected these to strengthen the argument. Then, as nothing could be more different than the refraction of the upper surfaces of these glasses, I examined the three sets of rings that were formed by these three combinations, and found them so perfectly alike that it was not possible to perceive any difference in their size and colour. This shows that the first surface of the incum-

bent glasses merely acts as an inlet to the rays that afterwards form the rings.

To confirm the idea that the mere admission of light would be sufficient, I used a slip of glass polished on one side but roughened with emery on the other; this being laid upon a 21-feet object-glass, I saw a set of rings through the rough surface; and though they appeared hazy, they were otherwise complete in figure and colour. The slip of glass when laid in the same manner upon the letters of a book made them appear equally hazy; so that the rings were probably as sharply formed as the letters.

Having now already great reason to believe that no modification, that can be given by the first surface to the incident rays of light, is essential to the formation of the rings, I made the following decisive experiment.

Upon a small piece of looking-glass I laid half a double convex lens of 16-inches focus, with the fracture exposed to the light, as represented in figure 11. Under the edge of the perfect part of the lens was put a small lump of wax, soft enough to allow a gentle pressure to bring the point of contact towards the fractured edge, and to keep it there. In this arrangement it has already been shown that there are two different ways of seeing two sets of rings: by the rays 1, 2, 3, we see a primary set; and by 1, 2, 4, 5, the secondary set belonging to it: by the rays 6, 7, 2, 3 we see a different primary set; and by 6, 7, 2, 4, 5, we see its secondary one. That this theory is well founded has already been proved; but if we should have a doubt remaining, the interposition of any small opaque object upon the looking-glass near the fracture will instantly stop the latter two sets of rings, and show

the alternate colours of the two sets that will then be seen by the rays 1, 2, 3, and 1, 2, 4, 5. Remove in the next place the stop from the looking-glass, and bring the second shadow of the penknife over the primary set, and there will then only remain the two sets of rings formed by incident rays which come from 6, and which have never passed through the upper surface of the lens. Now, as both sets of rings in this case are completely formed by rays transmitted upwards from the coated part of the looking-glass without passing through the first surface of the incumbent lens, the proof that the modifying power of that surface is not required to the formation of the rings is established.

It can hardly be supposed that the first surface of the lens should have any concern in the formation of the rings when the rays are reflected from the looking-glass towards the eye; but the same experiment, that has proved that this surface was not required to be used with incident rays, will show that we may do without it when they are on their return. We need only invert the fractured lens, as in figure 12, when either the rays 1, 2, 4, 5, or 6, 7, 2, 4, 5, will convey the image of the rings after their formation to the eye without passing through any part of the lens.

XXV. *Of the Action of the second Surface.*

As rings are formed when two glasses are laid upon each other, it is but reasonable to expect that the two surfaces at least which are placed together should have an immediate effect upon them, and so much the more, as it has been ascertained that the first surface assists only by permitting light to pass into the body of the glass. Some of the experiments

that have been instituted for examining the action of the first surface will equally serve for investigating that of the second.

The lens already used with a strong emery scratch being again placed on the mirror, but with the injured side downwards, I found that the rings, when brought under the scratch, were not distorted; they had only a black mark of the same shape as the scratch across them.

The lens with a scabrous side was also placed again upon the mirror, but with the highly polished side upwards. In this position the scabrousness of the lowest surface occasioned great irregularity among the rings, which were indented and broken wherever the little polished holes that make up a scabrous surface came near them; and if by gently lifting the lens a strong contact was prevented, the colours of the rings were likewise extremely disfigured and changed.

As we have now seen that a polished defect upon the second surface will affect the figure of the rings that are under them, it will remain to be determined whether such defects do really distort them by some modification they give to the rays of light in their passage through them, or whether they only represent the rings as deformed, because we see them through a distorted medium. For although the scabrousness did not sensibly affect the figure of the rings when it was on the first surface, we may suppose the little polished holes to have a much stronger effect in distorting the appearance of the rings when they are close to them. The following experiment will entirely clear up this point.

Over the middle of a 22-inch double convex lens I drew a strong line with a diamond, and gave it a polish afterwards that it might occasion an irregular refraction. This being

prepared, I laid a slip of glass upon a plain metalline mirror, and placed the lens with the polished line downwards upon the slip of glass. This arrangement has been shown to give two sets of rings. When I examined the primary set, a strong disfiguring of the rings was visible; they had the appearance of having been forced asunder, or swelled out, so as to be much broader one way than another. The rings of the secondary set had exactly the same defects, which being strongly marked, could not be mistaken. The centers of the two sets, as usual, were of opposite colours, the first being black, the second white; and all those defects that were of one colour in the first set, were of the opposite colour in the second. When, by the usual method, I changed the colours of the centers of the rings, making that of the primary white and of the secondary black, the defects in both set were still exactly alike, and as before; except that they had also undergone the like transformation of colour, each having assumed its opposite. It remains now only to show that this experiment is decisive; for by the established course of the rays we saw the secondary set of rings when it had a white center by the transmitted rays marked 1, 2, 4, 5, in figure 13; and when it had a black one, by the reflected rays 6, 7, 2, 4, 5, of the same figure; but in neither of these two cases did the rays come through the defective part of the lens in their return to the eye.

This experiment proves more than we might at first be aware of; for it does not only establish that the second surface, when properly combined with a third surface, has a modifying power whereby it can interrupt the regularity of the rings, but also one whereby it contributes to their formation;

for, if it can give an irregular figure to them by transmitting its irregularly modified rays, it follows, that when these rays are regularly modified it will be the cause of the regular figure of the rings. Nay, it proves more; for if it modifies the figure of the rings by transmission, it modifies them no less by reflection; which may be seen by following the course of the rays 6, 7, 2, 4, 5; for as they do not pass through the defective place of the lens, they can only receive their modification from it by reflection. This opens a field of view to us that leads to the cause of all these intricate phænomena, of which in a second part of this paper I shall avail myself.

XXVI. *Of the Action of the third Surface.*

When a double convex lens is laid upon a plain metalline mirror that happens to have an emery scratch in its surface, we see it as a black line under the rings that are formed over them. This shows, that when a defect from want of polish has not a power to reflect light in an irregular manner, it cannot distort the rings that are formed upon it.

When I laid a good 21-feet object glass upon a plain slip that had some defects in its surface, the rings, in every part of the object glass that was brought over them, were always disfigured; which proves that a reflection from a defective third surface has a power of forming distorted rings, and that consequently a reflection from one that is perfect must have a power of forming rings without distortion, when it is combined with a proper second surface.

When the defective slip of glass, with a perfect lens upon it, was placed upon a metalline mirror, I saw the secondary set affected by distortions of the rings that were perfectly like

those in the primary set; which proves that a polished defect in the third surface will give modifications to the rays that form the rings by transmission as well as by reflection.

XXVII. *The Colour of the reflecting and transmitting Surfaces is of no consequence.*

I laid seven 54-inch double convex lenses upon seven coloured pieces of plain glass. The colours of the glasses were those which are given by a prism, namely, violet, indigo, blue, green, yellow, orange, and red. The rings reflected from each of these glasses were in every respect alike; at least so far that I could have a black, a white, a red, an orange, a yellow, a green, or a blue center with every one of them, according to the degree of pressure I used. The lenses being very transparent, it may be admitted that the colours of the glasses seen through them would in some degree mix with the colours of the rings; but the action of the cause that gives the rings was not in the least affected by that circumstance.

I saw the rings also by direct transmission through all the coloured glasess except a dark red, which stopped so much light that I could not perceive them. The colour of the glasses, in this way, coming directly to the eye, gave a strong tinge to the centers of the rings, so that instead of a pure white I had a bluish-white, a greenish white, and so of the rest; but the form of the rings was no less perfect on that account.

XXVIII. *Of the Action of the fourth Surface.*

We have already seen that a set of rings may be completely formed by reflection from a third surface, without

the introduction of a fourth; this, at all events, must prove that such a surface is not essential to the formation of rings, but as not only in direct transmission, but also when two sets of rings are to be seen, one of which may be formed by transmission this fourth surface must be introduced; I have ascertained by the following experiments how far the same has any share in the formation of rings.

In direct transmission, where the light comes from below, the fourth surface will take the part which is acted by the first, when rings are seen reflected from a metalline mirror. Its office therefore will be merely to afford an entrance to the rays of light into the substance of the subjacent glass; but when that light is admitted through the first, second, and third surfaces, the fourth takes the office of a reflector, and sends it back towards the point of contact. It will not be required to examine this reflection, since the light thus turned back again is, with respect to the point of contact, in the same situation in which it was after its entrance through the first surface when it proceeded to the same point; but when two sets of rings are to be formed by rays, either coming through this point directly towards the fourth surface, or by reflection from the same point towards the place where the secondary rings are to be seen, it will then be necessary to examine whether this surface has any share in their formation, or whether these rings, being already completely formed, are only reflected by it to the eye. With a view to this, I selected a certain polished defect in the surface of a piece of coach-glass, and when a 26-inch lens was laid upon it, the rings of the set it produced were much distorted. The lens was then put upon a perfect slip of glass, and both together were laid

upon the defective place of the coach-glass. The rings of the secondary set reflected by it were nevertheless as perfect as those of the primary set. It occurred to me that these rings might possibly be reflected from the lowest surface of the perfect slip of glass, especially as by lifting it up from the coach-glass I still continued to see both sets. To clear up this point, therefore, I took away the slip, and turning the defective place of the coach-glass downwards, produced a set of perfect rings between the lens and the upper surface of the coach-glass, and brought it into such a situation that a secondary set must be reflected from the defective place of the lowest surface. This being obtained, the rings of this set were again as well formed and as free from distortions as those of the primary set.

Upon a plain metalline mirror I laid down two lenses, one a plano-convex, the other a plano concave, both of 2,9 inches focus, and having the plain side upwards. When two 21-inch double convex glasses were laid upon them, the secondary sets of both the combinations were of equal size, and perfectly like their primary sets; which proves that the refraction of the fourth surface is either not at all concerned, or at least has so little an effect in altering the size of the rings that it cannot be perceived.

The result of the foregoing experiments, relating to the action of the several surfaces, is,

I. That only two of them are essential to the formation of concentric rings.

II. That these two must be of a certain regular construction, and so as to form a central contact.

III. That the rays from one side or the other, must either

pass through the point of contact, or through one of the surfaces about the same point to the other to be reflected from it.

IV. And that in all these cases a set of rings will be formed, having their common center in the place where the two surfaces touch each other.

XXIX. *Considerations that relate to the Cause of the formation of concentric Rings.*

It is perfectly evident that the phænomena of concentric rings must have an adequate cause, either in the very nature or motion of the rays of light, or in the modifications that are given to them by the two essential surfaces that act upon them at the time of the formation of the rings.

This seems to reduce the cause we are looking for to an alternative that may be determined; for if it can be shown that a disposition of the rays of light to be alternately reflected and transmitted cannot account for the phænomena which this hypothesis is to explain, a proposition of accounting for them by modifications that may be proved, even on the very principles of Sir I. Newton to have an existence, will find a ready admittance. I propose, therefore, now to give some arguments, which will remove an obstacle to the investigation of the real cause of the formation of the concentric rings; for after the very plausible supposition of the alternate fits, which agrees so wonderfully well with a number of facts that have been related, it will hardly be attempted, if these should be set aside, to ascribe some other inherent property to the rays of light, whereby we might account for them; and thus we shall be at liberty to turn our thoughts

to a cause that may be found in the modifications arising from the action of the surfaces which have been proved to be the only essential ones in the formation of rings.

XXX. *Concentric Rings cannot be formed by an alternate Reflection and Transmission of the Rays of Light.*

One of the most simple methods of obtaining a set of concentric rings is to lay a convex lens on a plain metalline mirror; but in this case we can have no transmission of rays, and therefore we cannot have an alternate reflection and transmission of them. If to get over this objection it should be said that, instead of transmission, we ought to substitute absorption; since those rays which in glass would have been transmitted will be absorbed by the metal, we may admit the elusion; it ought however to have been made a part of the hypothesis.

XXXI. *Alternate Fits of easy Reflection and easy Transmission, if they exist, do not exert themselves according to various Thicknesses of thin Plates of Air.*

In the following experiment, I placed a plain well polished piece of glass 5,6 inches long, and 2,3 thick, upon a plain metalline mirror of the same length with the glass; and in order to keep the mirror and glass at a distance from each other, I laid between them, at one end, a narrow strip of such paper as we commonly put between prints. The thickness of that which I used was the 640th part of an inch; for 128 folds of them laid together would hardly make up two-tenths. Upon the glass I put a 39-inch double convex lens; and having

exposed this combination to a proper light, I saw two complete sets of coloured rings.

In this arrangement, the rays which convey the secondary set of rings to the eye must pass through a thin wedge of air, and if these rays are endowed with permanent fits of easy reflection, and easy transmission, or absorption, their exertion, according to Sir I. NEWTON, should be repeated at every different thickness of the plate of air, which amounts to the $\frac{1}{89652}$ part of an inch, of which he says "Hæc est crassitudo aeris in primo annulo obscuro radiis ad perpendiculum incidentibus exibito, qua parte is annulus obscurissimus est." The length of the thin wedge of air, reckoned from the line of contact, to the beginning of the interposed strip of paper, is 5,2 inches, from which we calculate that it will have the above mentioned thickness at $\frac{1}{27}$ of an inch from the contact; and therefore at $\frac{1}{54}$, $\frac{3}{54}$, $\frac{5}{54}$, $\frac{7}{54}$, $\frac{9}{54}$, $\frac{11}{54}$, &c. we shall have the thickness of air between the mirror and glass, equal to $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, &c. of which the same author says that they give "crassitudines Aeris in omnibus Annulis lucidis, qua parte illi lucidissimi sunt." Hence it follows that, according to the above hypothesis, the rings of the secondary set which extended over a space of ,14 of an inch, should suffer more than seven interruptions of shape and colour in the direction of the wedge of air.

In order to ascertain whether such an effect had any existence, I viewed the secondary set of rings upon every part of the glass-plate, by moving the convex lens from one end of it gradually to the other; and my attention being particularly directed to the 3d, 4th, and 5th rings, which were extremely

distinct, I saw them retain their shape and colour all the time without the smallest alteration.

The same experiment was repeated with a piece of plain glass instead of the metalline mirror, in order to give room for the fits of easy transmission, if they existed, to exert themselves; but the result was still the same; and the constancy of the brightness and colours of the rings of the secondary set, plainly proved that the rays of light were not affected by the thickness of the plate of air through which they passed.

XXXII. *Alternate Fits of easy Reflection and easy Transmission, if they exist, do not exert themselves according to various Thicknesses of thin Plates of Glass.*

I selected a well polished plate of coach glass 17 inches long, and about 9 broad. Its thickness at one end was 33, and at the other 31 two-hundredths of an inch; so that in its whole length it differed $\frac{1}{100}$ of an inch in thickeness. By measuring many other parts of the plate I found that it was very regularly tapering from one end to the other. This plate, with a double convex lens of 55 inches laid upon it, being placed upon a small metalline mirror, and properly exposed to the light, gave me the usual two sets of rings. In the secondary set, which was the object of my attention, I counted twelve rings, and estimated the central space between them to be about $1\frac{1}{3}$ times as broad as the space taken up by the 12 rings on either side; the whole of the space taken up may therefore be reckoned equal to the breadth of 40 rings of a mean size: for the 12 rings, as usual, were gradually contracted in breadth as they receded from the center, and,

by a measure of the whole space thus taken up, I found that the breadth of a ring of a mean size was about the 308th part of an inch.

Now, according to Sir I. NEWTON's calculation of the action of the fits of easy reflection and easy transmission in thick glass plates, an alternation from a reflecting to a transmitting fit requires a difference of $\frac{1}{137545}$ part of an inch in thickness ;* and by calculation this difference took place in the glass plate that was used at every 80th part of an inch of its whole length; the 12 rings, as well as the central colour of the secondary set, should consequently have been broken by the exertion of the fits at every 80th part of an inch; and from the space over which these rings extended, which was about ,13 inch, we find that there must have been more than ten such interruptions or breaks in a set of which the 308th part was plainly to be distinguished. But when I drew the glass plate gently over the small mirror, keeping the secondary set of rings in view, I found their shape and colour always completely well formed.

This experiment was also repeated with a small plain glass instead of the metalline mirror put under the large plate. In this manner it still gave the same result, with no other difference but that only six rings could be distinctly seen in the secondary set, on account of the inferior reflection of the subjacent glass.

* NEWTON's Optics, p. 277.

XXXIII. *Coloured Rings may be completely formed without the Assistance of any thin or thick Plates, either of Glass or of Air.*

The experiment I am now to relate was at first intended to be reserved for the second part of this paper, because it properly belongs to the subject of the flection of the rays of light, which is not at present under consideration; but as it particularly opposes the admission of alternate fits of easy reflection and easy transmission of these rays in their passage through plates of air or glass, by proving that their assistance in the formation of rings is not required, and also throws light upon a subject that has at different times been considered by some of our most acute experimentalists, I have used it at present, though only in one of the various arrangements, in which I shall have occasion to recur to it hereafter.

Sir I. NEWTON placed a concave glass mirror at double its focal length from a chart, and observed that the reflection of a beam of light admitted into a dark room, when thrown upon this mirror, gave "four or five concentric irises or rings of colours like "rainbows."* He accounts for them by alternate fits of easy reflection and easy transmission exerted in their passage through the glass-plate of the concave mirror.†

The Duke De CHAULNES concluded from his own experiments of the same phenomena, "that these coloured rings depended upon "the first surface of the mirror, and that the "second surface, or that which reflects them after they had "passed the first, only served to collect them and throw them

* NEWTON'S Optics, p. 265. † Ibid, p. 277.

" upon the pasteboard, in a quantity sufficient to make them " visible."*

Mr. BROUGHAM, after having considered what the two authors I have mentioned had done, says, " that upon the whole " there appears every reason to believe that the rings are " formed by the first surface out of the light which, after re- " flection from the second surface, is scattered, and passes on " to the chart."†

My own experiment is as follows. I placed a highly polished 7 feet mirror, but of metal instead of glass, that I might not have two surfaces, at the distance of 14 feet from a white screen, and through a hole in the middle of it one-tenth of an inch in diameter I admitted a beam of the sun into my dark room, directed so as to fall perpendicularly on the mirror. In this arrangement the whole screen remained perfectly free from light, because the focus of all the rays which came to the mirror was by reflection thrown back into the hole through which they entered. When all was duly prepared, I made an assistant strew some hair-powder with a puff into the beam of light, while I kept my attention fixed upon the screen. As soon as the hair-powder reached the beam of light the screen was suddenly covered with the most beautiful arrangement of concentric circles displaying all the brilliant colours of the rainbow. A great variety in the size of the rings was obtained by making the assistant strew the powder into the beam at a greater distance from the mirror; for the rings contract by an increase of the distance, and dilate on a nearer approach of the powder.

* PRIESTLEY's History, &c. on the Colours of thin Plates, p. 515.

† Phil. Trans. for 1796, p. 216.

This experiment is so simple, and points out the general causes of the rings which are here produced in so plain a manner, that we may confidently say they arise from the flection of the rays of light on the particles of the floating powder, modified by the curvature of the reflecting surface of the mirror.

Here we have no interposed plate of glass of a given thickness between one surface and another, that might produce the colours by reflecting some rays of light and transmitting others; and if we were inclined to look upon the distance of the particles of the floating powder from the mirror as plates of air, it would not be possible to assign any certain thickness to them, since these particles may be spread in the beam of light over a considerable space, and perhaps none of them will be exactly at the same distance from the mirror.

I shall not enter into a further analysis of this experiment, as the only purpose for which it is given in this place is to show that the principle of thin or thick plates, either of air or glass, on which the rays might alternately exert their fits of easy reflection and easy transmission, must be given up, and that the fits themselves of course cannot be shown to have any existence.

XXXIV. *Conclusion.*

It will hardly be necessary to say, that all the theory relating to the size of the parts of natural bodies and their interstices, which Sir I. NEWTON has founded upon the existence of fits of easy reflection and easy transmission, exerted differently, according to the different thickness of the thin plates of which he supposes the parts of natural bodies to consist, will remain unsupported; for if the above mentioned fits have no existence,

Fig. 1.

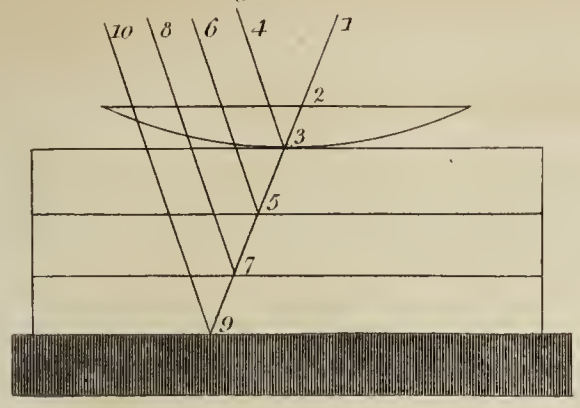


Fig. 2.

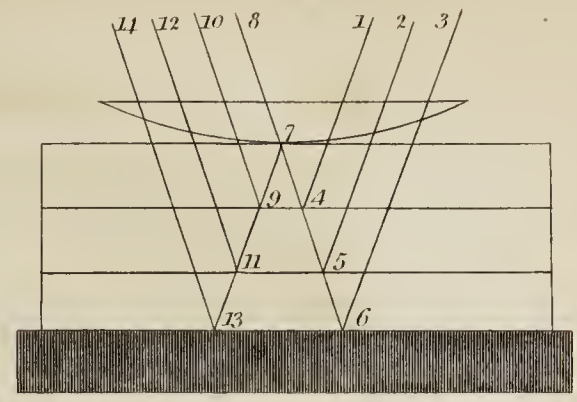


Fig. 3.

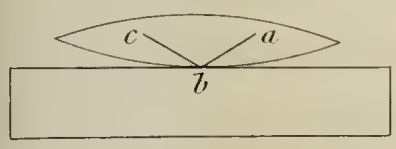


Fig. 4.

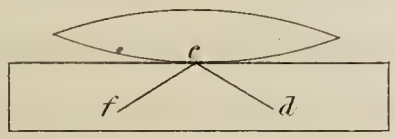


Fig. 5.

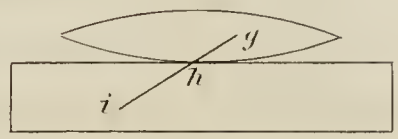


Fig. 6.

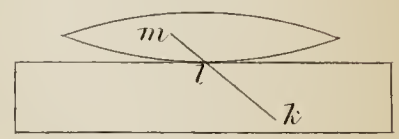


Fig. 7.

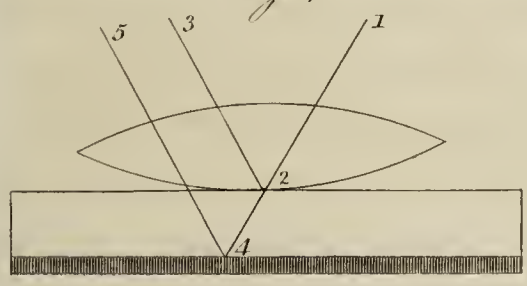


Fig. 8.

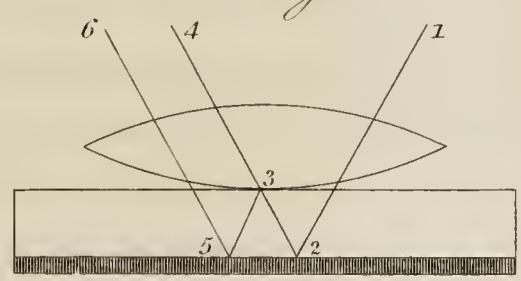


Fig. 9.

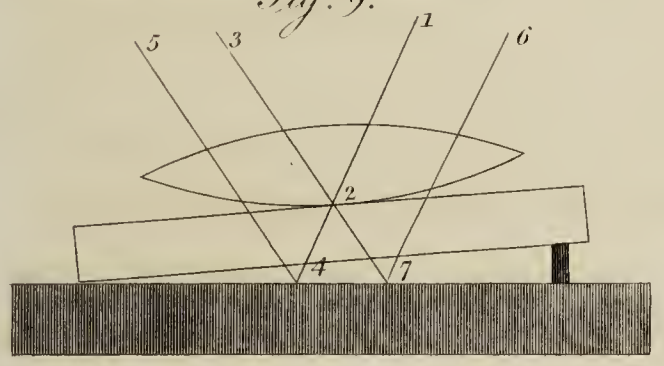


Fig. 10.

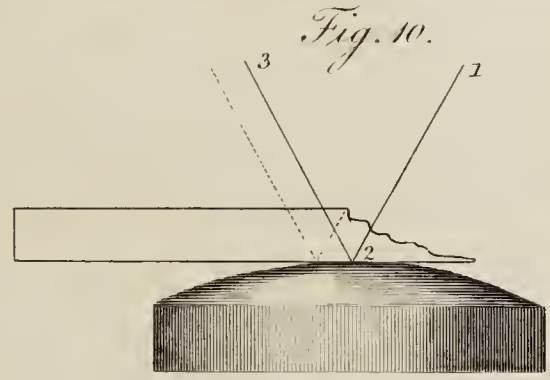


Fig. 11.

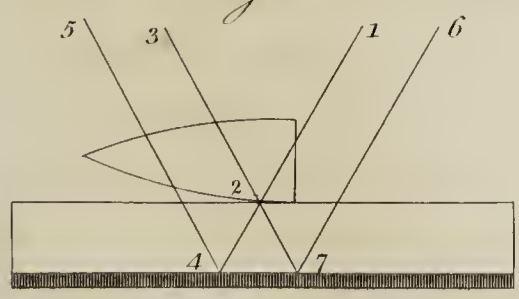


Fig. 12.

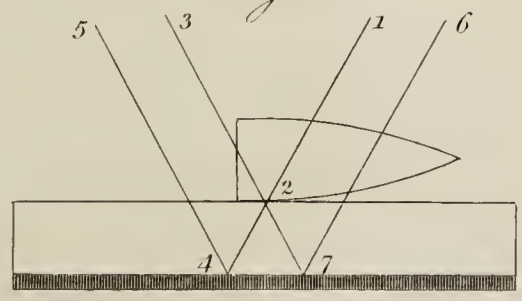
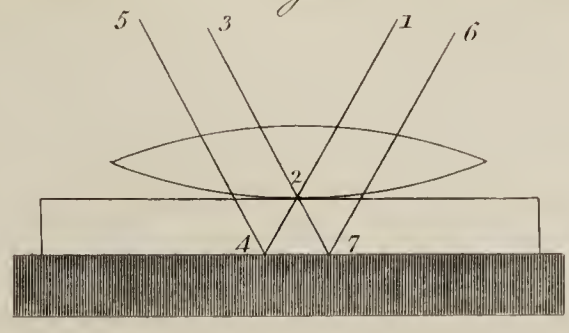


Fig. 13.



the whole foundation on which the theory of the size of such parts is placed, will be taken away, and we shall consequently have to look out for a more firm basis on which a similar edifice may be placed. That there is such a one we cannot doubt, and what I have already said will lead us to look for it in the modifying power which the two surfaces, that have been proved to be essential to the formation of rings, exert upon the rays of light. The Second Part of this Paper, therefore, will enter into an examination of the various modifications that light receives in its approach to, entrance into, or passage by, differently disposed surfaces or bodies; in order to discover, if possible, which of them may be the immediate cause of the coloured rings that are formed between glasses.

X. *On the Economy of Bees. In a Letter from* Thomas Andrew Knight, *Esq. F. R. S. to the Right Honourable Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 14, 1807.

My dear Sir,

In the prosecution of those experiments on trees, accounts of which you have so often done me the honour to present to the Royal Society, my residence has necessarily been almost wholly confined to the same spot; and I have thence been induced to pay considerable attention to the economy of bees, amongst other objects; and as some interesting circumstances in the habit of these singular insects appear to have come under my observation, and to have escaped the notice of former writers, I take the liberty to communicate my observations to you.

It is, I believe, generally supposed that each hive, or swarm, of these insects remains at all times wholly unconnected with other colonies in the vicinity; and that the bee never distinguishes a stranger from an enemy. The circumstances which I shall proceed to state will, however, tend to prove that these opinions are not well founded, and that a friendly intercourse not unfrequently takes place between different colonies, and is productive of very important consequences in their political economy.

Passing through one of my orchards rather late in the evening in the month of August, in the year 1801, I observed that several bees passed me in a direct line from the hives in my own garden to those in the garden of a cottager, which was about a hundred yards distant from it. As it was considerably later in the evening than the time when bees usually cease to labour, I concluded that something more than ordinary was going forward. Going first to my own garden, and then to that of the cottager, I found a very considerable degree of bustle and agitation to prevail in one hive in each: every bee, as it arrived, seemed to be stopt and questioned, at the mouth of each hive; but I could not discover any thing like actual resistance, or hostility, to take place; though I was much inclined to believe the intercourse between the hives to be hostile and predatory. The same kind of intercourse continued, in a greater or less degree, during eight succeeding days, and though I watched them very closely, nothing occurred to induce me to suppose that their intercourse was not of an amicable kind. On the tenth morning, however, their friendship ended, as sudden and violent friendships often do, in a quarrel; and they fought most furiously; and after this there was no more visiting.

Two years subsequent to this period I observed the same kind of intercourse to take place between two hives of my own bees, which were situated about two hundred yards distant from each other: they passed from each hive to the other just as they did in the preceding instance, and a similar degree of agitation was observable. In this instance, however, their friendship appeared to be of much shorter duration, for they

fought most desperately on the fifth day; and then, as in the last mentioned case, all further visiting ceased.

I have some reason to believe that the kind of intercourse I have described, which I have often seen, and which is by no means uncommon, not unfrequently ends in a junction of the two swarms; for one instance came under my observation, many years ago, in which the labouring bees, under circumstances perfectly similar to those I have described, wholly disappeared, leaving the drones in peaceable possession of the hive, but without any thing to live upon. I have also reasons for believing, that whenever a junction of two swarms, with their property, is agreed upon, that which proposes to remove, immediately, or soon afterwards, unites with the other swarm, and returns to the deserted hive during the day only to carry off the honey: for having examined at night a hive from which I suspected the bees to be migrating, I found it without a a single inhabitant. I was led to make the examination by information I had received from a very accurate observer, that all the bees would then be absent. A very considerable quantity of honey was in this instance left in the hive without any guards to defend it; but I conclude that the bees would have returned for it, had it remained till the next day. Whenever the bees quit their habitation, in this way, I have always observed some fighting to take place; but I conceived it to be between the bees of the adjoining hives, and those which were removing; the former being attracted by the scent of the honey, which the latter were carrying off.

On the farm which I occupy, there were formerly many old decayed trees, the cavities of which were frequently occupied

by swarms of bees; and when these were destroyed, a board was generally fitted to the aperture which had been made to extract the honey; and the cavity was thus prepared for the reception of another swarm, in the succeeding season. Whenever a swarm came, I constantly observed, that about fourteen days previous to their arrival a small number of bees, varying from twenty to fifty, were every day employed in examining, and apparently in keeping possession of the cavity; for if molested, they showed evident signs of displeasure, though they never employed their stings in defending their proposed habitation. Their examination was not confined to the cavity, but extended to the external parts of the tree above; and every dead knot particularly arrested their attention; as if they had been apprehensive of being injured by moisture which this might admit into the cavity below; and they apparently did not leave any part of the bark near the cavity unexamined. A part of the colony, which purposed to emigrate, appeared in this case to have been delegated to search for a proper habitation; and the individual who succeeded must have apparently had some means of conveying information of his success to others; for it cannot be supposed that fifty bees should each accidentally meet at, and fix upon, the same cavity, at a mile distant from their hive; which I have frequently observed them to do, in a wood where several trees were adapted for their reception; and indeed I observed that they almost uniformly selected that cavity which I thought best adapted to their use.

It not unfrequently happened that swarms of my own bees took possession of these cavities, and such swarms were in several instances followed from my garden to the trees: and they were observed to deviate very little from the direct line

between the one point and the other; which seems to indicate that those bees which had formely acted as purveyors, now became guides.

Two instances came under my own observation in which a swarm was received into a cavity, of which another swarm had previous possession. In the first instance I arrived with the swarm, and I could not discover that the least opposition was made to their entrance: in the second instance, observing the direction that the swarm took, I used all the expedition I could to arrive first at the tree, to which I supposed they were going, whilst a servant followed them; and a descent of ground being in my favour, and the wind against them, I succeeded in arriving at the tree some seconds before them; and I am perfectly confident that not the least resistance was opposed to their entrance.

Now it does not appear probable that animals so much attached to their property as bees are, so jealous of all approach towards it, and so ready to sacrifice their lives in defence of it, should suffer a colony of strangers, with whose intentions they were unacquainted, to take possession, without making some effort to defend it: nor does it seem much more probable that the same animals, which spent so much time in examining their future habitation, in the cases I have mentioned, should have attempted in this case to enter without knowing whether there was space sufficient to contain them, and without any examination at all. I must therefore infer, that some previous intercourse had taken place between the two swarms, and that those in the possession of the cavities were not unacquainted with the intentions of their guests; though the formation of any thing like an agreement between the different

parties be scarcely consistent with the limitations generally supposed to be fixed by nature to the instinctive powers of the brute creation.

Brutes have evidently language; but it is a language of passion only, and not of ideas. They express to each other sentiments of love, of fear, and of anger; but they appear to be wholly incapable of transmitting to each other any ideas they have received from the impression of external objects. They convey to other animals of their species, on the approach of an enemy, a sentiment of danger; but they appear wholly incapable of communicating what the enemy is, or the kind of danger apprehended. A language of more extensive use seems, from the preceding circumstances, to have been given to bees; and if it be not, in some degree, a language of ideas, it appears to be something very similar.

When a swarm of bees issue from the parent hive, they generally soon settle on some neighbouring bush or tree; and as in this situation they are generally not at all defended from rain or cold, it is often inferred that they are less amply gifted with those instinctive powers, that direct to self-preservation, than many other animals. But their object in settling soon after they leave the hive is apparently nothing more than to collect their numbers; and they have generally, I believe always, another place to which they intend subsequently to go: and if the situation they select be not perfectly adapted to secure them from injuries, it is probably, in almost all instances, the best they can discover. For I have very often observed that when one of my hives was nearly ready to swarm, one of the hollow trees I have mentioned (and generally that best adapted for the accommodation of a swarm) was every day occupied by

a small number of bees; but that after the swarm had issued from that hive, and had taken possession of another, the tree was wholly deserted; whence I inferred that the swarm, which would have taken possession of the cavity of that tree, had relinquished their intended migration, when a hive was offered them at home. And I am much disposed to doubt, whether it be not rather habit, produced by domestication, during many successive generations, than any thing inherent in the nature of bees, which induces them to accept a hive, when offered them, in preference to the situation they have previously chosen: for I have noticed the disposition to migrate to exist in a much greater degree in some families of bees than in others; and the offspring of domesticated animals inherit, in a very remarkable manner, the acquired habits of their parents. In all animals this is observable; but in the dog it exists to a wonderful extent; and the offspring appears to inherit not only the passions and propensities, but even the resentments, of the family from which it springs. I ascertained by repeated experiment that a terrier, whose parents had been in the habit of fighting with polecats, will instantly shew every mark of anger when he first perceives the scent of that animal; though the animal itself be wholly concealed from his sight. A young spaniel brought up with the terriers shewed no marks whatever of emotion at the scent of the polecat; but it pursued a woodcock, the first time it saw one, with clamour and exultation: and a young pointer, which I am certain had never seen a partridge, stood trembling with anxiety, its eyes fixed, and its muscles rigid, when conducted into the midst of a covey of those birds. Yet each of these dogs are mere varieties of the same species; and to that species none of these habits are

given by nature. The peculiarities of character can therefore be traced to no other source than the acquired habits of the parents, which are inherited by the offspring, and become what I shall call instinctive hereditary propensities. These propensities, or modifications of the natural instinctive powers of animals, are capable of endless variation and change; and hence their habits soon become adapted to different countries and different states of domestication, the acquired habits of the parents being transferred hereditarily to the offspring. Bees, like other animals, are probably susceptible of these changes of habit, and thence, when accustomed through many generations to the hive, in a country which does not afford hollow trees, or other habitations adapted to their purpose, they may become more dependent on man, and rely on his care wholly for an habitation; but in situations where the cavities of trees present to them the means of providing for themselves, I have found that they will discover such trees in the closest recesses of the woods, and at an extraordinary distance from their hives; and that they will keep possession of such cavities in the manner I have stated: and I am confident that, under such circumstances, a swarm never issues from the parent hive, without having previously selected some such place to retire to.

It has been remarked by Mr. JOHN HUNTER, that the matter which bees carry on their thighs is the farina of plants with which they feed their young, and not the substance with which they make their combs; and his statement is, I believe, perfectly correct: but I have observed, that they will also carry other things on their thighs. I frequently covered the decorticated parts of trees, on which I was making experiments, with a cement composed of bees-wax and turpentine;

and in the autumn I have frequently observed a great number of bees employed in carrying off this substance. They detached it from the tree with their forceps, and the little portion thus obtained was then transferred by the first to the second leg, by which it was deposited on the thigh of the third: the farina of plants is collected and transferred in the same manner. This mixture of wax and turpentine did not, however, appear to have been employed in the formation of combs; but only to attach the hive to the board on which it was placed, and probably to exclude other insects, and air during winter. Whilst the bees were employed in the collection of this substance, I had many opportunities of observing the peaceful and patient disposition of them as individuals, which Mr. HUNTER has also, in some measure, noticed. When one bee had collected its load, and was just prepared to take flight, another often came behind it, and despoiled it of all it had collected. A second, and even a third, load was collected and lost in the same manner, and still the patient insect pursued its labour, without betraying any symptoms of impatience, or resentment. When, however, the hive is approached, the bee appears often to be the most irritable of all animals; but a circumstance I have observed amongst another species of insects, whose habits are in many respects similar to those of bees, induces me to believe, that the readiness of the bees to attack those who approach their hives, does not in any degree spring either from the sense of injury or apprehensions of the individual, who makes the attack. If a nest of wasps be approached without alarming its inhabitants, and all communication be suddenly cut off between those out of the nest, and those within it, no provocation will induce the former to defend their nest,

or themselves. But if one escape from within, it comes with a very different temper, and appears commissioned to avenge public wrongs, and prepared to sacrifice its life in the execution of its orders. I discovered the circumstance, that wasps thus excluded from their nest would neither defend it, nor themselves, at a very early period of my life; and I profited so often, by the discovery, as a school boy, that I am quite certain of the fact I state; and I do not entertain any doubt, though I speak from experiments less accurately made, that the actions of bees, under similar circumstances, would be the same.*

Mr. HUNTER conceived bees wax to be an animal substance, which exuded between the scales of the belly of the insect; but I am strongly disposed to believe that it is collected from plants, and merely desposited between the scales of the belly

* A curious circumstance, relative to wasps, attracted the notice of some of my friends last year, and has not, I believe, been satisfactorily accounted for. A greater number of female wasps were observed in different parts of the kingdom, in the spring and early part of the summer of that year, than at almost any former period; yet scarcely any nests, or labouring wasps, were seen in the following autumn; the cause of which I believe I can explain. Attending to some peach trees in my garden, late in the autumn of the year 1805, on which I had been making experiments, I noticed, during many successive days, a vast number of female wasps, which appeared to have been attracted there by the shelter and warmth of a south wall; but I did not observe any males. At length, during a warm gleam in the middle of one of the days, a single male appeared, and selected a female close to me; and this was the only male I saw in that season. The male wasp, which is readily distinguishable from the female and labourer, by his long antennæ and shining wings, and by a blacker and more slender body, is rarely seen out of the nest, except in very warm days, like the drone bee; and the nests of wasps, though very abundant in the year 1805, were not formed till remarkably late in the season; and thence I conclude that the males had not acquired maturity till the weather had ceased to be warm, and that the females, in consequence, retired to their long winter sleep without having had any intercourse with them.

of the bee, for the joint purposes of being carried with convenience, and giving the the temperature necessary for being moulded into combs: and I am led to this conclusion, not only by the circumstance of wax being found in the vegetable world, but also by having often observed bees employed in detaching something from the bases of the leaves of plants with their forceps, which they did not deposit on their thighs, as they do (I believe invariably) the farina of plants. I have also frequently observed the combs of very late swarms to be remarkably thin and white, and brittle; which are circumstances very favourable to the conclusion that the wax is a vegetable substance, for it would probably be less abundant during autumn than in summer; and that portion which had remained on the plants till late in the season would hence become more colourless by exposure to light, as well as more dry and brittle, than when at first exuded; but were it an animal substance, there does not appear any reason why it should be more dry and brittle, or less abundant, in the autumn, than in the spring and summer. The conclusions of Mr. HUNTER are, however, always drawn with so much caution, and he united so much skill and science with the greatest degree of industry, that it is not without much hesitation and diffidence, that I venture to put my opinion in opposition to his authority.

T. A. KNIGHT.

Elton, May 4, 1807.

XI. *Observations and Measurements of the Planet Vesta.* By John Jerome Schroeter, *F. R. S.*

TRANSLATED FROM THE GERMAN.

Read May 28, 1807.

At our very first observations with magnifying powers of 150 and 300 applied to the excellent new 15-feet reflector, we found the planet Vesta *without any appearance of a disc*, merely as a point like a fixed star with an intense, radiating light, and exactly of the same appearance as that of any fixed star of the sixth magnitude. In the same manner we both afterwards saw this planet several times with our naked eyes, when the sky was clear, and when it was surrounded by smaller invisible stars, which precluded all possibility of mistaking it for another. This proves how very like the intense light of this planet is to that of a fixed star.

As the observations and measurements of Ceres, Pallas, and Juno, were made with the same eye-glasses but with the 13-feet reflector, we soon after compared the planet Vesta with the same glasses of 136 and 288 times magnifying power in the 13-feet reflector. In both these telescopes its image was, *without the least difference*, that of a fixed star of the 6th magnitude with an intense radiating light; so that this new planet may with the greatest propriety be called an *asteroid*.

April 26th in the evening at 9 o'clock, true time, I succeeded

in effecting the measurement of Vesta, with the same power of 288 by means of the 13-feet reflector, with which that of Ceres, Pallas, and Juno had been made; and when viewed by this reflector it also appeared exactly in the same manner. Of several illuminated discs, of 2,0 to 0,5 decimal lines, which I had before made use of for measuring the satellites of Saturn and Jupiter, the smallest disc only of 0,5 lines could be used for this purpose; by it the rounded nucleus of the planet Vesta, when the disc was at the distance of 611,0 lines from the eye, appeared *at most* of the same size, and I must even estimate its diameter as $\frac{1}{6}$ smaller. If therefore, we attend, not to the full magnitude of the projection, but the estimation just mentioned, it follows by calculation that the *apparent diameter of the planet Vesta* is *only* 0,488 *seconds* and consequently only *half* of what I have found to be the apparent diameter *of the fourth satellite of Saturn.*

This extraordinary smallness, with such an intense, radiant and unsteady light of a fixed star, is the more remarkable, as, according to the preliminary calculations of Dr. GAUSS, there can be no doubt that this planet is found in the same region between Mars and Jupiter, in which Ceres, Pallas, and Juno perform their revolutions round the sun; that, in close union with them, it has the same cosmological origin; and that as a planet of such smallness and of so very intense light, it is comparatively *near to the earth.* This remarkable circumstance will no doubt be productive of important cosmological observations, as soon as the elements of the new planet have been sufficiently determined, and its distance from the earth ascertained by calculation.

Lilienthal, May 12, 1807.

XII. *A new Eudiometer, accompanied with Experiments, elucidating its Application.* *By* William Hasledine Pepys, *Esq.* *Communicated by* Charles Hatchett, *Esq. F. R. S.*

Read June 4, 1807.

THE important part which atmospheric air performs, in maintaining the principle of life in animals, in combustion of every description, the acidification, and oxidation of a great variety of substances, and in numerous other processes both of nature and art, gives a high degree of interest to every thing calculated to extend our knowledge of its nature and properties.

The evidence furnished by modern chemistry, of the existence of many other aeriform substances, increases this interest, especially when it is considered that, owing to their possessing some of the most, obvious properties of atmospheric air, as transparency, elasticity, and a power of great expansion, on being exposed to an encrease of temperature, they were with very few exceptions till lately, confounded either with common air, or not even suspected to exist.

When to these considerations, we add the facility with which some products, especially the gaseous, are evolved, in circumstances under which in the present state of our knowledge, we should hardly look for them; the power they possess of decomposing each other, and by an interchange and new arrangement of principles, of producing compounds, possessing

properties altogether different, from those of the ingredients supposed to be present; and the facilities which every new detection of unsuspected principles afford, towards the discovery of others, and consequently the composition, or analysis of bodies before held to be simple, it will not appear a matter of surprize, that the subject of eudiometry, should have obtained a considerable degree of attention from modern philosophers.

This would be an improper place to enumerate all that has been done, or proposed, by different men of eminence, towards the production of something like a perfect system on this important subject; yet some allusion to their labours appears to be indispensible, and will be the means of preventing some circumlocution in our farther progress.

HALES* appears to be the first who observed absorption to take place in common air, on mixing it with air obtained from a mixture of *Walton pyrites* and *spirits* of *nitre;* and that in this process, from being clear they became " a reddish turbid fume."

Dr. PRIESTLY, as he informs us in his *Observations on different kinds of Air*,† was much struck with this experiment, but never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to the Walton pyrites, till encouraged by a suggestion of Mr. CAVENDISH, that probably, the red appearance of the mixture depended upon the spirits of nitre only, he tried solutions of the different metals in that acid, and catching the air which was generated, obtained what he wished. To the air thus produced, he gave the name of *nitrous* air, and from its possessing the properties of absorbing

* Statical Essays, Vol. I. p. 224. Vol. II. p. 280.

† Phil. Trans. for 1772, p. 210.

that portion of atmospheric air which he calls *dephlogisticated*, first proposed its being used as a test for ascertaining the purity of air. His method of proceeding was ingenious and simple; known quantities of the air to be tried, and of nitrous gas being mixed, were admitted after the diminution of volume occasioned by their union, into a graduated tube, which he denominated a *eudiometer*.

It was with the test of nitrous gas that Mr. CAVENDISH* made his masterly analysis of the air at Kensington and London; and by many laborious processes and comparative trials obtained results, the accuracy of which has been more distinctly perceived the more the science of chemistry has advanced.

The slow combustion of phosphorus, which unites with the oxigene to form an acid, and the decomposition of the fluid sulphuret of potash, are certain methods of separating combinations consisting of oxigene and azote: but the decomposition is effected so slowly, by the action of these substances, that it became a desirable object, to discover some means for accelerating the process. This was supposed to have been effected by GUYTON, who proposed heating the sulphuret of potash; in doing this, sulphurated hydrogene gas however is frequently evolved, which, mixing with the residual gas, increases its quantity, and renders the result fallacious.

The green sulphate of iron impregnated with nitrous gas, first discovered by Dr. PRIESTLY, and recently used by Mr. DAVY for eudiometrical purposes, from its possessing the property of absorbing oxigene gas from the atmosphere, is much

* Phil. Trans. for 1783.

to be preferred to the method with nitrous gas, as the green sulphate of iron does not combine with the other gases, with which the nitrous gas is commonly found to be contaminated, and more certain results are obtained.

Having had occasion to repeat many of the experiments of others, and to make some new ones, I soon found what every one, who has been engaged on the same subject, must have experienced; that an apparatus more commodious than has yet been proposed, and at the same time capable of giving correct results, with the greatest minuteness, was still a desideratum in eudiometry. To detail the various ideas that presented themselves on the subject, would be an unnecessary encroachment on the time of this Society: but as I at last succeeded in contriving an instrument, possessing the above properties in a very eminent degree, I flatter myself I shall not be thought intrusive, in offering a description of it.

This apparatus, which is of easy construction, and extremely portable, consists of a glass measure M. fig. 1, graduated into hundred parts; a small gum elastic bottle B. fig. 2, capable of containing about twice the quantity of the measure, and furnished with a perforated glass stopper S, which is well secured in the neck of it, by means of waxed thread wound tight round it: and a glass tube, T. fig. 3, also graduated, but into tenths of the formed divisons, or into thousand parts of the measure.

The glass stopper, made fast in the neck of the gum elastic bottle, as above mentioned, has its exterior end ground with emery, exactly to fit the mouth of the measure; to the lower end of the graduated tube T is cemented a small steel cock,

which is secured into the neck of a very small gum elastic bottle, by means of waxed thread: S B. fig. 4; the other end of the tube is conical, so as to present a very small orifice.

Besides this, the apparatus is furnished with a kind of moveable cistern C, in which the tube can be slid easily up and down, and yet in such a manner that the water or other liquid in the cistern may not pass. This is easily accomplished by means of a cork fitted into its mouth, with a perforation through its axis to receive the tube. The cistern, when in use, is to be filled with water, or mercury, as the experiment may require, and becomes a secondary cistern for the measure, as will be more clearly understood, by the following description of the method of performing experiments with this instrument.

The measure is filled with the air, or gas, over mercury in the usual manner; and the elastic bottle is charged with the solution, intended to be employed as the reagent: the orifice of the stopper is then inserted into the mouth of the measure, in the mercury, and pressed home to its place.

The bottle and measure being thus united, are to be firmly held at the joint. Upon pressing the former, a portion of the fluid is injected into the latter, and the gas suffers a degree of compression, by which the action of the affinity, between it and the fluid, is accelerated. On taking off the pressure, the bottle, by its elasticity, endeavours to obtain its original form, and receives back the fluid. This process should be continued as long as any absorption is observed to take place. When absorption ceases, the bottle is to be separated from the measure under mercury, and the quicksilver which remains in the

measure being brought to the level of that in the cistern, the quantity of absorption is then to be determined, which is done as follows:

Suppose atmospheric air has been the subject of the experiment, and consequently a large residuum left: first note the hundred parts; and then to obtain a knowledge of the fractional parts, remove the measure into the small cistern, in which the graduated tube filled with mercury is placed: slide the tube above the surface of the fluid in the measure, and opening the stop-cock, suffer the mercury to descend till it has drawn the fluid in the measure to a regular division; then stop the cock, and register the hundred parts on the measure, and the thousand parts on the graduated tube; the united quantities give the sum of the residual gas. Observe well in registering the thousand parts, that the fluids are exactly on a level, on the outside and the inside of the measure; this may be easily effected, by pouring out a portion of the liquid of the small cistern, or adding thereto.

If instead of atmospheric air, a gas is tried, which so far as it is uncontaminated can be nearly wholly absorbed by the reagents employed, the process becomes exceedingly simple; for if the residuum is under a hundred part of the measure, it may be transferred completely into the graduated tube, and its quantity at once ascertained.

The stopper S would have injected the fluid with greater velocity had it been straight, but it would not then have been so convenient in the analysis of compound gases, where both mercury and hot solutions are occasionally employed, as the mercury would have so compressed the fluid in the bottle, in

introducing it under that metal, as to have thrown out a portion of its contents, and also have robbed the hot solutions of the temperature which was necessary for their perfect action.

As to the size of the measure M, I have generally preferred the cubic inch divided into hundred parts. This is easily effected by taking a stout glass tube about half an inch calibre, sealing one end, then weighing 3422 grains of mercury, equal to 252 grains of distilled water at temperature 50° FAHRENHEIT. This is introduced into the tube ; the extra length is cut off with a sharp-edged file, care being taken to leave a sufficient portion to grind the perforated stopper S into its mouth.

The divisions are obtained by a small measure, made from a glass tube sealed at the end, and cut off exactly to the hundred parts of a cubic inch, equal to 34.2 grains of mercury, which being ground flat, is stopped by a piece of plate glass, and the divisions marked by the diamond, upon the introduction of each hundred part of mercury into the measure M.

The tube T, is divided into tenths of the measure M, or thousand parts of a cubic inch. This is done by measuring one hundred part of a cubic inch into the tube, and dividing it into ten parts, marking the divisions with fluoric acid, or black enamel.

To prove the accuracy of the instrument, I shall proceed to relate a few experiments made with it.

The elastic bottle being filled with the solution of sulphate of iron impregnated with nitrous gas, and the measure with atmospheric air, they were united, and by gentle injection $\frac{215}{1000}$ were absorbed.

If the experiment is made hastily, the impregnated solution loses a portion of its nitrous gas, which must be again absorbed by a solution of green sulphate of iron.

For ascertaining the purity of nitrous gas, the bottle may be charged with the solution of green sulphate or muriate of iron.

For carbonic acid gas, with lime or barytic water.

For oxygene gas, with the solution of green sulphate of iron impregnated with nitrous gas.

For sulphurated hydrogene gas, a solution of nitrate of silver was put in the elastic bottle, and sulphurated hydrogene gas† into the graduated measure. Upon the first injection, the solution took a black flocculent appearance, and a considerable portion of the gas was absorbed. After repeating the process as before mentioned, the residuum was $\frac{5}{1000}$.

The instrument may be likewise generally applied to the analysis of mixed gases.

I have been able, completely to separate the carbonic acid gas from the sulphurated hydrogene, by a solution of the nitrate of silver, or of mercury employed hot. The carbonic acid gas is expanded in this process, but on standing over mercury it returns to its original volume. The sulphurated hydrogene, in this instance, is taken up by the metallic nitrate. It should be here observed that the acetite of lead must not be used, as the carbonic acid gas, even at a high temperature, decomposes it, forming carbonate of lead.

The propriety of using the solutions hot, will be seen,

* Obtained from oxymuriate of potash by heat.

† Obtained from sulphuret of potash by diluted muriatic acid, and collected and preserved with the greatest care.

when we recollect that the carbonic acid gas is soluble in the water of solution, at the common temperature of all these compounds.

Nitrous gas, and carbonic acid gas, may be separated by means of the hot solution of the green sulphate of iron. To effect this, heat a solution on a glass capsule over a spirit lamp until ebullition. Having filled the measure with the compound gas, charge the elastic bottle with the hot solution, and unite them. The nitrous gas in two or three injections will be absorbed, changing the colour of the solution, while the carbonic acid gas will be a little rarefied, but no absorption of it will take place.

Previous to these experiments on the compound gases, I had tried several on the carbonic acid, sulphurated hydrogene, and nitrous gases in their unmixed states. One hundred parts of pure alcohol at the common temperature will absorb 70 parts in volume of carbonic acid, and the same quantity of sulphurated hydrogene. Alcohol impregnated with the latter, precipitates the solutions of the nitrates of lead, silver, and mercury, of a dark-brown colour. Nitric acid of the specific gravity 1.4, and also of 1.25, absorbs carbonic acid gas, without any apparent change in the nitric acid. Sulphurated hydrogene gas is also absorbed by nitric acid, which occasions a slight milky cloud or precipitate therein.

The solutions of nitrates of barytes, strontian, and lime, absorb carbonic acid gas equal to half their volume, without any apparent alteration.

Solutions of nitrates of barytes, strontian, and lime, also absorb sulphurated hydrogene gas, equal to $\frac{6}{10}$ of their volume, with a slight change of colour; the solutions thus impregnated,

precipitate solutions of nitrates of mercury, and of silver, and acetite of lead, of a dark brown colour, and would be useful as chemical reagents.

Carbonic acid gas, as I have before stated, decomposes solutions of the acetite of lead, hot, or cold, forming a precipitate of carbonate of lead.

Carbonic acid gas is absorbed by the solution of the green sulphate of iron, under the temperature of 100° FAHRENHEIT: but this is only the action of the water of solution. If the temperature be near boiling, or above 180° FAHRENHEIT, the solution encreases the volume of the gas without the slightest absorption; after carbonic acid gas has in this way been treated with the hot solutions, it is still soluble in water at the common temperature, or in aqueous solutions of lime, or alkali.

Nitrous gas is absorbed by solution of sulphuret of potash, with a separation or formation of sulphur. Upon injecting the solution the sides of the measure take a milky appearance, which on the second injection is washed down, insoluble in the liquor. About 80 parts from 100 of gas are absorbed.

Nitrous gas is also absorbed by nitrate of copper in solution, without any peculiar alteration.

In these experiments, great care must be taken not to encrease the temperature of the gas by the hand. To prevent this I use a pair of small circular-mouthed forceps, lined with cloth, which firmly grasp the measure, fig. 5; and if the experiments should in any way be delayed, a corresponding manometer will always be sufficient to correct the error occasioned by change of atmospheric temperature and pressure.

To ascertain the quantity of carbonic acid gas, contained in oxygene gas (of a known purity,) after combustion, or de-

composition of carbonaceous substances, lime water will be found sufficient.

If it is required to know the purity of the oxygene gas, after the carbonic acid gas has been absorbed, the best method and the least liable to error, is to withdraw the residual oxygene gas, by means of the small graduated tube before described.

To do this, remove the measure into the small cistern of mercury; press the quicksilver out of the small bottle by the fingers and thumb, and let the tube rise a sufficient height within the measure, that the bottle extending itself shall withdraw the whole of the gas from the measure, taking care that the cock be stopped as soon as it has completed it, and also to prevent the solution from entering the tube.

If the opening of the tube is small, it may then be drawn down into the mercury, without the possibility of any portion of the gas escaping, while the measure is dried or cleaned, or a fresh one filled with mercury supplied to receive it.

This way of transferring will be found very advantageous, particularly in the separation of gases, liable to be absorbed under certain temperatures, and also where a new series of re-agents are to be employed, as from the depositions of former solutions on the glass measure, a source of considerable error would arise.

The residual oxygene gas being thus transferred into a clean dry measure, the processes before described for examining oxygene gas may be then used; or the quantity of carbonic acid gas (for examination) being found by lime water, another measure of the gas may be tried, first with the green sulphate of iron impregnated with nitrous gas, and then with the green sulphate in solution only: these will take up both the carbonic

acid gas, and the oxygene gas, leaving only such residual gas as the oxygene might have originally contained.

Transferring is not here necessary, as the two solutions may be used one after the other, taking care to use the solution of green sulphate last.

Where it is not requisite to transfer the gas into a dried or clean measure, previous to the use of another solution, as in the instance I have just mentioned, a quantity of the first solution may be withdrawn, by simply filling the elastic bottle with mercury, then joining it to the measure, and by inclining the measure, the mercury by its gravity will displace the former solution.

If at any time the gas should get drawn into the elastic bottle, it may be very easily returned into the measure, by inclining sometimes the bottle, and sometimes the measure. The only error that could arise from this is, an increase of temperature in the gas, which may be rectified, by plunging the whole apparatus into mercury or water, of the standard temperature.

The advantages of this construction of the eudiometer, will be readily perceived by all those who are in the habit of making chemical experiments. The portion of gas to be examined is completely under command; it may be agitated without the least fear of the intrusion of any atmospheric air, and the process thereby very materially shortened. The gum elastic is a substance so little acted upon by chemical agents, that a great variety may be employed; and above all, we can very conveniently use hot solutions, which will be found an important auxiliary in the examination of some compound gases.

Fig. 1.



Fig. 3.

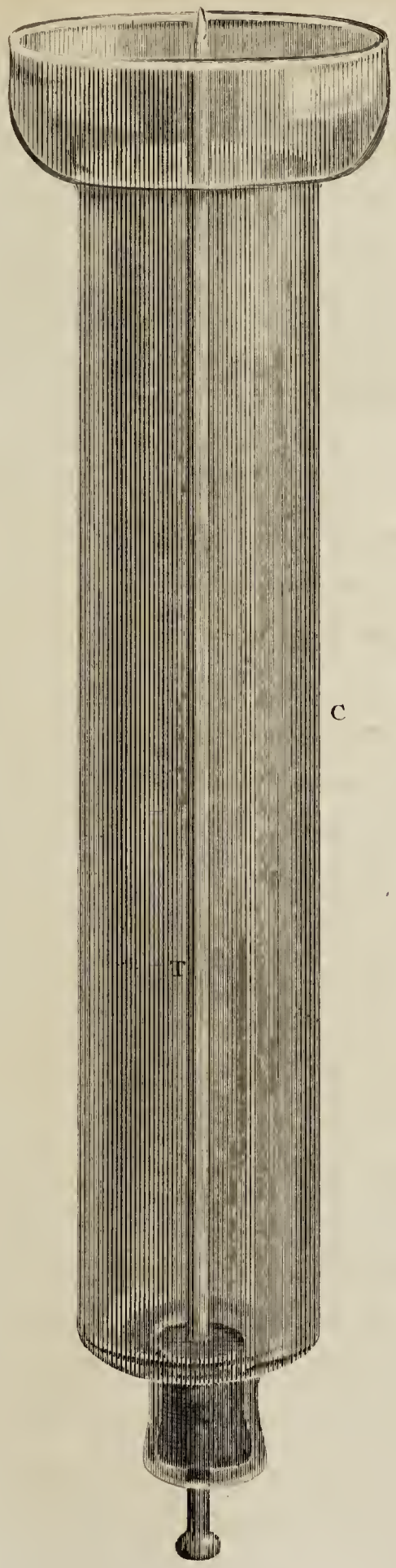


Fig. 2.



Fig. 4.

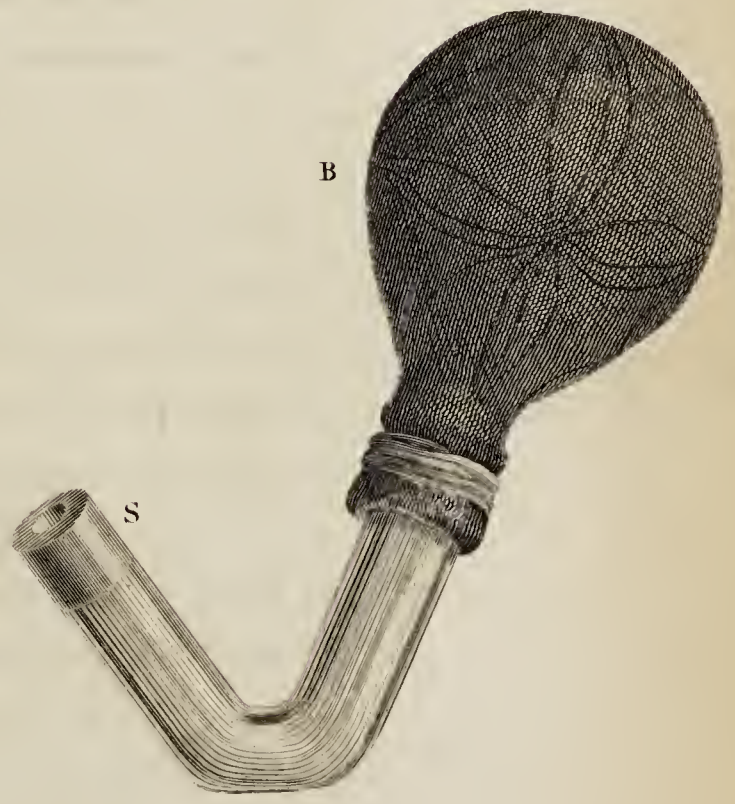
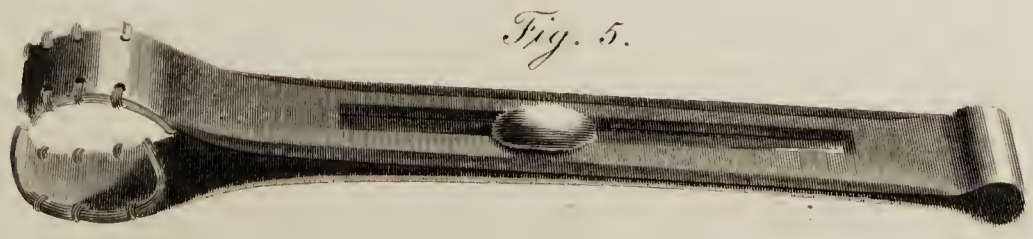


Fig. 5.



Silv. Bevan del. *Busire Sc.*

Simple as this instrument may appear, it is calculated to extend our knowledge of the different kinds of air, by the precision and accuracy which it enables us to obtain, and which solely constitutes the value of every experiment. A degree of confidence is inspired from knowing that we can depend upon our results; and hence much valuable time, which would have been wasted in uncertain, if not useless investigations, may be directly applied to the advancement of science.

XIII. *Observations on the Nature of the new celestial Body discovered by Dr.* Olbers, *and of the Comet which was expected to appear last January in its return from the Sun. By* William Herschel, *L. L. D. F. R. S.*

Read June 4, 1807.

THE late discovery of an additional body belonging to the solar system, by Dr. OLBERS, having been communicated to me the 20th of April, an event of such consequence engaged my immediate attention. In the evening of the same day I tried to discover its situation by the information I had obtained of its motion; but the brightness of the moon, which was near the full, and at no great distance from the object for which I looked, would not permit a star of even the 5th magnitude to be seen, and it was not till the 24th that a tolerable view could be obtained of that space of the heavens in which our new wanderer was pursuing its hitherto unknown path.

As soon as I found that small stars might be perceived, I made several delineations of certain telescopic constellations, the first of which was as represented in figure 1, and I fixed upon the star A, as most likely, from its expected situation and brightness, to be the one I was looking for. The stars in this figure, as well as in all the other delineations I had made, were carefully examined with several magnifying powers, that in case any one of them should hereafter appear to have

been the lately discovered object, I might not lose the opportunity of an early acquaintance with its condition. An observation of the star marked A, in particular, was made with a very distinct magnifying power of 460, and says, that it had nothing in its appearance that differed from what we see in other stars of the same size; indeed Dr. OLBERS, by mentioning in the communication which I received, that with such magnifying powers as he could use it was not to be distinguished from a fixed star,* had already prepared me to expect the newly discovered heavenly body to be a valuable addition to our increasing catalogue of asteroids.

The 25th of April I looked over my delineations of the preceding evening and found no material difference in the situation of the stars I had marked for examination; and in addition to them new asterisms were prepared, but on account of the retarded motion of the new star, which was drawing towards a period of its retrogradation, the small change of its situation was not sufficiently marked to be readily perceived the next day when these asterisms were again examined, which it is well known can only be done with night-glasses of a very low magnifying power.

A long interruption of bad weather would not permit any regular examination of the situation of small stars; and it was only when I had obtained a more precise information from the Astronomer Royal, who, by means of fixed instruments, was already in possession of the place and rate of motion of the new star, that I could direct my telescope with greater accu-

* Der neue planet zeigt sich als ein stern zwischen der 5ten und 6ten grösse und ist im fernrohr, wenigsten mit den vergrösserungen die ich anwenden kann, von einen fixstern nicht zu unterscheiden.

racy by an application of higher magnifying powers. My observations on the nature of this second new star discovered by Dr. OLBERS are as follow.

April 24. This day, as we have already seen, the new celestial object was examined with a high power; and since a magnifier of 460 would not show it to be different from the stars of an equal apparent brightness; its diameter must be extremely small, and we may reasonably expect it to be an asteroid.

May 21. With a double eye-piece magnifying only 75 times the supposed asteroid A makes a right-angled triangle with two small stars *a b*. See fig. 2.

With a very ditinct magnifier of 460 there is no appearance of any planetary disk.

May 22. The new star has moved away from *a b*, and is now situated as in fig. 3. The star A of figure 1 is no longer in the place where I observed it the 24th of April, and was therefore the asteroid. I examined it now with gradually increased magnifying powers, and the air being remarkably clear, I saw it very distinctly with 460, 577, and 636. On comparing its appearance with these powers alternately to that of equal stars, among which was the 463d of BODE'S Catalogue of the stars in the Lion of the 7th magnitude, I could not find any difference in the visible size of their disks.

By the estimations of the distances of double stars, contained in the first and second classes of the catalogues I have given of them, it will be seen that I have always considered every star as having a visible, though spurious, disk or diameter; and in a late paper I have entered at large into the method of detecting real disks from spurious ones; it may therefore be

supposed that I proceeded now with Vesta (which name I understand Dr. OLBERS has given the asteroid), as I did before in the investigation of the magnitudes of Ceres, Pallas, and Juno.

The same telescopes, the same comparative views, by which the smallness of the latter three had been proved, convinced me now that I had before me a similar fourth celestial body.

The disk of the asteroid which I saw was clear, well defined, and free from nebulosity. At the first view I was inclined to believe it a real one; and the Georgian planet being conveniently situated so that a telescope might without loss of time be turned alternately either to this or to the asteroid, I found that the disk of the latter, if it were real, would be about one-sixth of the former, when viewed with a magnifying power of 460. The spurious nature of the asteroidal disk, however, was soon manifested by an increase of the magnifying power, which would not proportionally increase its diameter as it increased that of the planet; and a real disk of the asteroid still remains unseen with a power of 636.

May 23. The new star has advanced, and its motion is direct; its situation with respect to the two small stars *a b*, is given in figure 4.

Its apparent disk with a magnifier of 460 is about 5 or 6-tenths of a second; but this is evidently a spurious appearance, because higher powers destroy the proportion it bears to a real disk when equally magnified. The air is not sufficiently pure this evening to use large telescopes.

May 24. With a magnifying power of 577 I compared the appearance of the Georgian planet to that of the asteroid, and with this power the diameter of the visible disk of the latter

was about one 9th or 10th part of the former. The apparent disk of the small star near β Leonis, which has been mentioned before, had an equal comparative magnitude, and probably the disks of the asteroid and of the star it resembles are equally spurious.

The 20 feet reflector, with many different magnifying powers, gave still the same result; and being already convinced of the impossibility, in the present situation of the asteroid, which is above two months past the opposition, to obtain a better view of its diameter, I used this instrument chiefly to ascertain whether any nebulosity or atmosphere might be seen about it. For this purpose the valuable quantity of light collected by an aperture of $18\frac{3}{4}$ inches directly received by an eye-glass of the front-view without a second reflection, proved of eminent use, and gave me the diameter of this asteroid intirely free from all nebulous or atmospheric appearances.

The result of these observations is, that we now are in possession of a formerly unknown species of celestial bodies, which by their smallness and considerable deviation from the path in which the planets move, are in no danger of disturbing, or being disturbed by them; and the great success that has already attended the pursuit of the celebrated discoverers of Ceres, Pallas, Juno, and Vesta, will induce us to hope that some further light may soon be thrown upon this new and most interesting branch of astronomy.

Observations of the expected Comet.

The comet which has been seen descending to the sun, and from the motion of which it was concluded that we should probably see it again on its return from the perihelion, was

expected to make its reappearance about the middle of last January, near the southern parts of the constellation of the whale.

January 27. Towards the evening, on my return from Bath, where I had been a few days, I gave my sister CAROLINA the place where this comet might be looked for, and between flying clouds, the same evening about 6^h 49′ she saw it just long enough to make a short sketch of its situation.

January 31. Clouds having obscured the sky till this time, I obtained a transitory view of the comet, and perceived that it was within a few degrees of the place which had been assigned to it; the unfavourable state of the atmosphere, however, would not permit the use of any instrument proper for examining it minutely.

There will be no occasion for my giving a more particular account of its place, than that it was very near the electrometer of the constellation, which in Mr. BODE's maps is called *machina electrica*; the only intention I had in looking for it, being to make a few observations upon its physical condition.

February 1. The comet had moved but very little from the place where it was last night; and as the air was pretty clear, I used a 10-feet reflector with a low power to examine it. There was no visible nucleus, nor did the light which is called the coma increase suddenly towards the centre, but was of an irregular round form, and with this low power extended to about 5, 6, or 7 minutes in diameter. When I magnified 169 times it was considerably reduced in size, which plainly indicated that a farther increase of magnifying power would be of no service for discovering a nucleus. On account of cloudy

weather I never had an opportunity of seeing the comet afterwards.

When I compare these observations with my former ones of 15 other telescopic comets, I find that out of the 16 which I have examined, 14 have been without any visible solid body in their centre, and that the other two had a very ill defined small central light, which might perhaps be called a nucleus, but did not deserve the name of a disk.

Fig. 1.



Fig. 2.

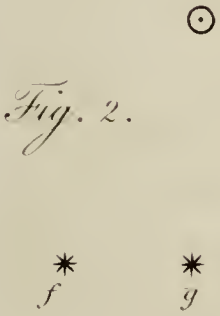


Fig. 3.



Fig. 4.



XIV. *On the Quantity of Carbon in carbonic Acid, and on the Nature of the Diamond. By* William Allen, *Esq. F. L. S. and* William Hasledine Pepys, *Esq. Communicated by* Humphry Davy, *Esq. Sec. R. S. M. R. I. A.*

Read June 18, 1807.

THE estimates of the quantity of real carbon in carbonic acid differing very widely, and the experiments of GUYTON DE MORVEAU upon the combustion of the diamond, detailed in the 31st volume of the *Annales de Chimie*, being liable to some objections from the manner in which the operations were conducted, we determined to institute a set of experiments, in order, if possible, to settle the question.

LAVOISIER, from the result of experiments apparently conducted with much accuracy, concluded that every hundred parts by weight of carbonic acid consisted of 28 carbon and 72 oxygene. This was in a great degree confirmed by the very valuable researches of SMITHSON TENNANT, Esq. on the nature of the diamond, an account of which is printed in the Transactions of this Society for the year 1797, and which were made previously to the experiments of GUYTON; but notwithstanding this, the result of GUYTON's experiment, which only allowed 17,88 per cent. of carbon to carbonic acid, has been adopted in all the systems of chemistry to the present time.

In researches of this nature, the results are much influenced by slight variations in the quality of the gas; but having had repeated experience of the accuracy of the eudiometer described

in No. XII. of this volume, we were enabled to proceed in this respect with great confidence.

Our object was, to consume certain known quantities of diamond and other carbonaceous substances in oxygene gas, and we at first determined to employ the sun's rays, by means of a powerful lens; but considering the uncertainty of a favourable opportunity in this country, and at the season in which our experiments were made, we resolved to employ the apparatus represented by the drawing.

Description of the Apparatus.

This consisted of two mercurial gasometers, fig. 1 and 2, each capable of containing from 70 to 80 cubic inches of gas. The internal cylinder CC is of cast iron, and solid, except the perforation through its middle; the external cylinder is also of cast iron; and the glass receiver slides up and down in the space between them, which is filled with mercury: not more than sixteen pounds are required for each, and the small bath B, fig. 1.

To the top of each receiver, a graduated scale or register H, is screwed, shewing the number of cubic inches of gas, measuring from the upper edge of the external iron cylinder. The level of the mercury is ascertained by a small glass gauge. The registers were graduated by throwing up one cubic inch of gas at a time.

The gasometers stand upon mahogany stools, perforated for a socket, to which, according to the nature of the experiment, a small receiver R, or the triple socket TS, or any other combination, may be united.

P represents the platina tube with its furnance; the ends

of the tube are mounted with female screws of brass, to one of which the acommodating screw socket AS was joined.

T is a double section of the platina tray which contained the substances to be heated. During their combustion, it was made to slide easily within the platina tube P. The accommodating socket and platina tray, are drawn considerably larger in proportion than the instrument.

By means of the triple socket and the cocks, the gas was made to pass freely over the substances in combustion, from one gasometer to the other; and by shutting off the communication with the platina tube, while that with the small receiver was open, any portion of gas in the gasometer, fig. 1. might be transferred into eudiometers or measures standing in the mercury bath M, for examination.

In order to discover whether the several sockets were air-tight, after the apparatus was put together, the communication with the gasometer, fig. 1, was closed, and the other communications opened; the receiver of the gasometer, fig. 2, being raised, drew up a column of mercury in the small receiver R, equal to two inches: the communication with the gasometer was then closed, and the column was supported without alteration. This was always tried previous to, and after every experiment. As the joints would bear this degree of exhaustion, we were confident they would resist a much greater pressure than we had any occasion to employ. The glass tubes GG, which connected the platina tube with the gasometers, enabled us to observe any flash arising from the combustion of hydrogene which might be contained in the substances subjected to experiment. In order to avoid prolixity, we shall generally state the method which was invariably followed.

We soon found that oxygene gas, even when secured in bottles with ground glass stoppers, was not always to be depended upon, but was sensibly deteriorated by keeping; and therefore in all our experiments we made the gas within an hour or two of the time of using it, and always from the hyperoxygenised muriate of potash. Its degree of purity was constantly ascertained by the eudiometer before every experiment, and was generally determined in about 10 minutes. The solution employed was that recommended by Professor DAVY; namely, the solution of green sulphate of iron saturated with nitrous gas;* and whenever the diminution had arrived at its maximum, and the gas began to increase in volume, we substituted a simple solution of the green sulphate of iron for that saturated with nitrous gas, and always had the most satisfactory results: for the simple sulphate absorbs any nitrous gas which may have escaped from the saturated solution, and the residuum in this case enables us to ascertain exactly the quantity of oxygene contained in the gas.

We determined to make our first experiment with charcoal, and as MOROZZO and ROUPPE had ascertained the absorbing properties of this substance, and as our results must obviously be influenced by it, our attention was directed to this point, the following quantities of different kinds of wood, sawed into slips $\frac{3}{16}$ of an inch were weighed.

White Fir,	300 grains.
Lignum Vitæ,	800
Box - -	400
Beech - -	500
English Oak,	250
Mahogany -	200

* This solution absorbs oxygene much more rapidly in warm weather than in cold.

These slips were put into small crucibles, and completely covered with dry sand. Heat was very gradually applied at first, until the volatile parts were dissipated; they were then kept about 40 minutes in a white heat. On being collected and weighed, while still warm, the charcoal from each was as follows:

Fir, - -	54,5 grs.	equal to	18,17 per cent.
Lignum Vitæ,	138	- -	17,25
Box -	81	- -	20,25
Beech -	75	- -	15
Oak -	43,5	- -	17,40
Mahogany	31,5	- -	15,75

These being exposed to the air during one week, increased in weight thus:

Fir, - -	13 per cent.
Lignum Vitæ,	9,6
Box - -	14
Beech -	16,3
Oak - -	16,5
Mahogany	18

Certain quantities being *confined* in common air increased very little in weight, and all in the same proportion; we are therefore much inclined to think that *this* increase is owing to an absorption of water from the air; and we repeatedly found that the greatest increase of weight took place in the first hour or two after exposure, and arrived at its maximum in less than 24 hours, as the following experiment, selected from several others, will prove.

40 grains of charcoal from willow wood, which had been put into a bottle with a ground glass stopper *immediately* after they

were removed from the fire, were exposed in the scale of a delicate balance, in a room where the thermometer was 62° FAHRENHEIT, barometer 30,26.

	Grains.		Total Increase.	Time.
6 o'clock P. M.	40			
½ past -	40,7	+ ,7		
7 - -	41,3	+ ,6 =	1,3	1 hour.
½ past -	41,6	+ ,3 =	1,6	1½ hours.
8 - -	41,8	+ ,2 =	1,8	2 hours.

The pieces were now spread out on paper after every weighing, to expose them more completely.

½ past 8 -	42,5	+ ,7 =	2,5	2½ hours.
9 - -	42,8	+ ,3 =	2,8	3 hours.
½ past -	43,1	+ ,3 =	3,1	3½ hours.
10 - -	43,3	+ ,2 =	3,3	4 hours.
½ past -	43,4	+ ,1 =	3,4	4½ hours.

Here it was left all night.

10 A. M. -	45	+ 1,6 =	5	16 hours.
4 P. M. -	45			
6 - -	44,5	— ,5 =	4,5	24 hours.
9 - -	44,4	— ,1 =	4,4	27 honrs.

Next day.

½ past 8 A. M.	44,9	+ ,5 =	4,9	38½ hours.
½ past 1 P. M.	44,7	— ,2 =	4,7	43½ hours.
10 - -	44,5	— ,2 =	4,5	52 hours.

Hence charcoal seems to act as an hygrometer: its greatest increase was 5 grains on 40, or 12½ per cent. And in order to ascertain to what the increase of weight was owing, we put 27,25 grains of charcoal, which had been thus exposed, into a small bottle and tube connected with a receiver standing in

the mercury bath, the whole of the vessels being also filled with mercury in order to exclude common air. Heat applied by an ARGAND's lamp produced gas equal to about *half the bulk of the charcoal;* but as soon as the temperature of the mercury rose to 214° FAHRENHEIT, elastic fluid streamed from every piece of charcoal, which *quickly condensed*, and $1\frac{1}{4}$ inch of the tube was occupied with water. This proved that our suspicion of the increase of weight being principally attributable to water, was well founded.

The result of these, and other experiments, plainly pointed out the precautions which were necessary in order to obtain an accurate result with charcoal; for if we had weighed 4 grains of the charcoal a few hours after it was made, we should only in fact have had 3,5 grains of real charcoal, and our calculations would have been erroneous. To avoid this source of error, we subjected our charcoal to a red heat *immediately* before using it, and also weighed it as speedily as possible; in fact, while it was still warm. It may be proper to state, that our weights were such as we could thoroughly depend upon.

The volume of gas being so much influenced by temperature and pressure, these were noted during every experiment; and thermometer 60° FAHRENHEIT, barmometer 30°, were assumed as the standard. GAY LUSSAC remarks, that from 32° to 212° FAHRENHEIT, dry air expands 0,00208, or $\frac{1}{480}$ part of its bulk for every degree of the thermometer. DALTON makes it 0,000207, or $\frac{1}{483}$ part; we therefore divided the whole quantity of gas by 480, and multiplied the quotient by the degrees of difference under 60°.

It being of great consequence in these experiments to know

the *exact* weight of a given quantity of oxygene and carbonic acid gases, we resolved to examine for ourselves, whether the statements already given were quite correct, and accordingly made carbonic acid over mercury from Carrara marble and diluted sulphuric acid, which being tried with lime water in PEPYS's eudiometer, was all absorbed in 3 minutes except 1 part in 100. We used two charges of lime water, though one would have been sufficient.

A glass globe being exhausted by an excellent air pump, was exactly balanced on a beam sensible to a minute portion of a grain; then being screwed upon one of the glass receivers of the mercurial gasometer previously filled with carbonic acid gas, 21 cubic inches entered. The globe was now increased in weight by 10,2 grains. In order to be certain we repeated the experiment, with exactly the same results. The 21 cubic inches were to be brought to the mean temperature and pressure, as the thermometer stood at 44° FAHRENHEIT, the barometer 29,86.

21	480)21,00 (0,043	60°
,68 add for temp.	16	44
21,68	0,688 add for temp.	16 diff.

Correction for Pressure.

30 : 29,86 : : 21,68 : 21,58

The volume therefore at mean temperature and pressure would have been 21,58 cubic inches.

21,58 : 10,2 : : 100 : 47,26

Consequently 100 cubic inches of carbonic acid gas at mean temperature and pressure weigh 47,26 grains.

We next tried oxygene gas from the hyperoxygenised muriate of potash made over mercury, and which by the eudiometer left only a residuum of 2 parts in 100. The glass globe exhausted as before, and weighed, was screwed on to the glass receiver of the mercurial gasometer containing oxygene, and 21 cubic inches entered, by which it increased in weight 7,3 grains. This experiment was repeated with exactly the same result. The thermometer and barometer remaining the same, we take the volume as before corrected.

21,58 cubic inches.

21,58 : 7,3 : : 100 : 33,82

Then 100 cubic inches of oxygene gas at mean temperature and pressure weigh 33,82 grains. After these experiments, we examined DAVY's researches on nitrous oxyde, and had the satisfaction to find that his estimate, both of carbonic acid and oxygene gases, agreed almost exactly with ours.

The next point was to ascertain whether lime water would take the whole of the carbonic acid gas from a mixture with oxygene, or common air; we therefore mixed a known quantity of carbonic acid gas with a certain quantity of common air, and on trying it with our eudiometer and lime water, the *whole* of the carbonic acid gas was in a short time absorbed. We also found, that though the solution of green sulphate saturated with nitrous gas would not take up the whole of the carbonic acid gas, yet the simple green sulphate, merely by its water of solution, absorbed it very readily.

It may be proper to notice here, that though we repeatedly tried the oxygene procured from hyperoxygenised muriate of potash by the eudiometer and lime water, it never gave the least trace of carbonic acid.

Experiment with Charcoal from Box-wood.

The thermometer being at 42° FAHRENHEIT, barometer at 30,2, we kept some box-wood charcoal red hot for a considerable time under sand, and weighed 4 grains as expeditiously as possible; this being put into the platina tray, was pushed to the middle of the platina tube; the oxygene (made from hyperoxygenised muriate of potash over mercury) was contained in gasometer No. 1; No. 2 was empty. Every thing being adjusted and found perfectly air-tight, the communication with the small receiver R was closed, and the common air contained in the tubes and sockets, amounting only to 2,84 cubic inches, was driven out by a pressure of oxygene from gasometer No. 1: when several cubic inches had passed into gasometer No. 2, the gas was let out by opening the cock at the top of its glass receiver, and pressing it down; the cock being then closed, the gasometer No. 2, was completely empty, and the whole of the gas from No. 1 was driven through the tubes into No. 2, and back again. The common air having been previously withdrawn from the small receiver R, we tried the purity of our oxygene by the eudiometer in the manner before described, and found a residuum of 3 parts in 100: we then disengaged as much gas as reduced the quantity to 47 cubic inches by the register or scale; to this must be added the contents of the tubes and sockets 2,84 cubic inches, making the total quantity of oxygene employed 49,84 cubic inches.

Correction for Temperature.

49,84	480)49,84(0,103		60°	
1,85 for temp.	18		42	
51,69	1,854	add for temp.	18	diff.

Correction for Pressure.

30 : 30,2 : : 51,69 : 52,03.

The volume, therefore, at mean pressure and temperature, would have beeen 52,03 cubic inches.

We now lighted a fire in the small black lead furnace under the platina tube, and as soon as it became red hot, opened the cocks and passed the gas from No. 1 to No. 2, when the charcoal entered into vivid combustion, and heated the platina tube white hot. The operation was repeated many times during 6 or 7 minutes, by pressing alternately upon the glasses of the gasometers. Not the least flash of light was observable in the glass connecting tubes G G, nor the smallest appearance of moisture. The furnace being removed, the tube was now cooled by the application of wet cloths; and when all was reduced to the temperature of the room, we pressed upon the glass of gasometer No. 2, so as to force all the gas into No. 1. The cock below being closed, we tried the tubes, &c. and found them perfectly air-tight. We next unscrewed the tube and took out the platina tray; but it only contained a light white ash, somewhat resembling the shape of the pieces of charcoal, and weighing only ,02 of a grain. On observing the register of No. 1, it indicated exactly the quantity of gas that we began with, so that although 3,98 grains of charcoal had been dissolved, the volume of gas was *unaltered* by it; a circumstance which had been remarked before by Lavoisier. The small receiver R was now nearly full of mercury; the communication with the gasometer being opened, the large glass receiver was gently pressed upon, until several cubic inches were forced through the receiver R, and tube K, in order to clear the latter of common air. This being done, on trying our gas with the eudiometer and lime

water, 56 parts were absorbed out of 100. These of course were carbonic acid gas; the test for oxygene absorbed 41, and a residuum of 3 was left, which was exactly what we began with. This is a striking proof that nothing but carbonic acid was produced in the experiment.

100 : 56 : : 52,03 : 29,13.

Then 29,13 cubic inches of carbonic acid gas were produced.

100 : 47,26 : : 29,13 : 13,76.

These 29,13 cubic inches of carbonic acid gas would therefore weigh 13,76 grains.

The charcoal weighed	4	grains.
The residual white ash	0,02	
Charcoal consumed	3,98	grains.

Then if 13,76 grains, the weight of the carbonic acid produced, contain 3,98 of charcoal, 100 grains must contain 28, 92.

13,76 : 3,98 : : 100 : 28,92.

Then, according to this experiment, 100 grains of carbonic acid gas contain 28,92 charcoal.

The gas before the experiment consisted of

Oxygene	50,47 cubic inches.
Azote	1,56
	52,03

After the Experiment,

Carbonic acid	29,13 cubic inches.
Oxygene	21,34
Azote	1,56
	52,03

Now as the volume of gas was unaltered, it will be fair to consider the quantity of oxygene gas consumed as equal to the carbonic acid produced, or 29,13 cubic inches.

Then, if 100 cubic inches of oxygene weigh 33,82 grains, 29,13 cubic inches will weigh 9,85 grains.

100 : 33,82 : : 29,13 : 9,85.

The weight of oxygene consumed was therefore 9,85 grains.

Charcoal consumed - - -	3,98
Carbonic acid from this statement	13,83 grains.
Ditto by calculations on carb. acid gas	13,76
	,07

13,83 : 3,98 : : 100 : 28,77.

Thus, calculating by the oxygene consumed, 100 grains of carbonic acid gas contain 28,77 charcoal.

First Experiment on Diamond.

Thermometer 56° FAHRENHEIT, barometer 30,20.

Our oxygene was made as in the former experiment: it contained no carbonic acid; and on being tried with the impregnated green sulphate, left a residuum of 3 parts in 100.

Having selected nine of the clearest and most transparent Brazil diamonds, we found they weighed 3,95 grains. These were ranged in the platina tray, which was placed in the tube, and the whole apparatus, adjusted as before, was perfectly air-tight. The quantity of oxygene was 49,84 cubic inches, as in the last experiment. The same precautions were used to secure accuracy in the results as in the former experiment; and it would only be an unnecessary intrusion on the time of the Society to repeat them. The platina tube was heated red-hot,

and kept so for ten minutes; during this time the gas was repeatedly passed from one gasometer to the other; the tube did not become white hot, as in the experiment with charcoal, because in this case the combustion went on more slowly. When every thing was cooled to the temperature of the room, the gas was all passed into No. 1, by pressing down the receiver of No. 2, and the volume was precisely the same as when we began the experiment. On drawing out the tray, we observed that some of the diamonds were reduced to a minute speck, and all of them resembled opake white enamel: there was no discoloration in the tray, nor any residual ash whatever; the unconsumed parts weighed 1,46 grains; the original weight

was	3,95	
	1,46	
consequently	2,49	grains were consumed.

We could not perceive any dullness on the surface of the mercury in the gasometers, or any appearance of moisture.

On introducing lime water to 100 parts of the gas in the eudiometer, a dense white precipitate was formed, and 36 parts absorbed; the test for oxygene absorbed 60, and a residuum of 4 was left.

Correction for Temperature.

60°	480)49,84(0,103	49,84	
56	4	,41	add for temp.
4 difference.	,412	50,25	

Correction for Pressure.

30 : 30,20 : : 50,25 : 50,58.

The quantity of oxygene at the mean was 50,58 cubic inches.

100 : 36 : : 50,58 : 18,20 cubic inches.

The quantity of carbonic acid gas produced was 18,20 cubic inches.

100 : 47,26 : : 18,20 : : 8,60 grains.

8,60 : 2,49 : : 100 : 28,95.

Then 100 grains of carbonic acid gas contain 28,95 of diamond.

Calculation by Oxygene.

100 : 33,82 : : 18,20 : 6,15 grains of oxygene consumed

2,49 grains of diamond.

8,64

Calculation by carbonic acid 8,60

,04 difference.

8,64 : 2,49 : : 100 : 28,81.

Thus if we calculate upon the oxygene consumed, 100 grains of carbonic acid gas contain 28,81 of diamond.

Second Experiment on Diamond.

Thermometer 48° FAHRENHEIT, barometer 30,08. Oxygene gas made as usual, left a residuum of 3 parts in 100.

Eleven small diamonds, weighing 4,01 grains, were put into the tray. We began with 49,84 cubic inches of oxygene; and every thing being properly adjusted, kept the platina tube red-hot for a quarter of an hour, and during this time the gas was passed from one gasometer to the other, as in the former experiments. When the tubes, &c. were cooled down to the temperature of the room, all the gas was transferred to gasometer No. 1. and the volume was exactly the same as before the experiment. On examining the tray, all the diamonds were entirely consumed and not a vestige left.

Lime water absorbed 57,5 parts from 100.
The test for oxygene 39,5
Residuum - 3

100

Correction for Temperature.

60°	0,103	49,84
48	12	1,23
12 diff.	1,236 add for temp.	51,07

Correction for Pressure.

30 : 30,08 : : 51,07 : 51,20.

The volume of gas at the mean was therefore 51,20 cubic inches.

100 : 57,50 : : 51,20 : 29,44.

Then 29,44 cubic inches of carbonic acid gas were produced.

100 : 47,26 : : 29,44 : 13,91.

13,91 : 4,01 : : 100 : 28,82.

Then, according to this experiment, 100 grains of carbonic acidcontain 28,82 diamond.

Calculation by Oxygene.

100 : 33,82 : : 29,44 : 9,95 grains of oxygene consumed
4,01 of diamond.

13,96
Calculation by carbonic acid 13,91

,05 difference.

13,96 : 4,01 : : 100 : 28,72.

Then, calculating by the weight of oxygene employed, 100 grains of carbonic acid contain 28,72 diamond.

The precipitate in lime water from the gas produced in the combustion of diamond, appeared to us denser than that from the combustion of charcoal.

In order to see how far the weight of the precipitate of carbonate of lime would agreee with the results of the foregoing experiments, we drew off 20,5 cubic inches of the gas which had been thus altered by the combustion of diamond in the last experiment by the register H, and received it in bottles over mercury; then admitting lime water, we obtained a copious precipitate of carbonate of lime, which being dried at the temperature of 212° FAHRENHEIT, weighed 12 grains.

But as the 20,5 cubic inches require the same corrections to bring them to the mean temperature and pressure; we say, as the actual volume of all the gas is to its correction, so is the quantity drawn off to that which it would have been at the mean:

49,84 : 51,20 : : 20,50 : 21,06, the volume after the corrections were made.

Then, to find how much carbonic acid was contained in these 21,06 cubic inches, we state it thus: As the total quantity of gas after the experiment is to the total weight of carbonic acid gas found by calculation, so is the quantity of gas experimented upon to the weight of carbonic acid gas which it ought to have contained,

51,20 : 13,91 : : 21,06 : 5,72 grains.

Every 100 grains of precipitated carbonate of lime contain 44 grains of carbonic acid; 12 grains were procured in our experiment. 100 : 44 : : 12 : 5,28

Therefore the carbonic acid contained in our precipitate of 12 grains weighed 5,28; by calculation it should have weighed

5,72; this is as near as we had a right to expect from the difficulty of collecting the precipitate.

Stone Coal.

Upon the suggestion of our mutual friend Professor DAVY, we next examined the results of the combustion of stone coal and plumbago; thermometer 57° FAHRENHEIT, barometer 29,65.

The stone coal from Wales, employed by maltsters, is well known to contain little or no maltha or mineral pitch, and to burn without flame.

A portion of this coal was placed under sand in a crucible, and exposed to a strong heat for one hour; 4 grains of it thus prepared were put into the tray: our oxygene left a residuum of 5 parts in 100, and we began with 49,84 cubic inches as usual. The tray being placed in the platina tube was heated to redness for about 10 minutes. When the gas was first passed, we thought we saw a flash in the glass tubes. On suffering the whole to cool the quantity of gas still remained the same, and the tray being drawn out contained only ,5 of a grain unconsumed. From the gas thus charged with 3,5 grains of coal,

Lime water absorbed	53	parts from 100.
The tests for oxygene	39	
Residuum -	8	or an increase of 3.
	100	

Correction for Temperature.

60°	0,103	49,84
57	3	,30
3 diff.	0,309 add for temp.	50,14

Correction for Pressure,

30 : 29,65 : : 50,14 : 49,55.

The quantity of oxygene at the mean was therefore 49,55 cubic inches.

100 : 53 : : 49,55 : 26,26.

Consequently 26,26 cubic inches of carbonic acid gas were produced.

100 : 47,26 : : 26,26 : 12,41 grains.

12,41 : 3,50 : : 100 : 28,20.

Then, according to this experiment, 100 grains of carbonic acid gas contain 28,20 of coal.

Calculation by Oxygene.

100 : 33,82 : : 26,26 : 8,88 grains of oxygene consumed.

3,50 coal

12,38

Calculation by carbonic acid	12,41
by oxygene	12,38
difference	,03

Here, contrary to what happened in other experiments, the calculation by carbonic acid rather exceeds that by oxygene:

12,38 : 3,50 : : 100 : 28,27.

Calculating therefore by oxygene, 100 grains of carbonic acid contain 28,27 of coal.

Experiment with Plumbago.

Thermometer 44° FAHRENHEIT, barometer 29,94.

4 grains of plumbago, from a very fine specimen belonging to Dr. BABINGTON, were put into the tray. Our oxygene left

a residuum of 2 parts in 100, and we began with 49,84 cubic inches. The tray, with its contents, being placed in the platina tube, was heated to redness for a quarter of an hour, and the gas made to pass over it several times. When all was cool, the original quantity was neither increased nor diminished, and on withdrawing the tray we found only ,2 of a grain of oxide of iron; so that this specimen of plumbago contains only 5 per cent. oxide of iron.

The gas being now examined,

Lime water absorbed	55	parts from 100
The tests for oxygene	42	
Residuum - -	3	or an increase of 1 per cent.
	100	

Correction for Temperature.

60°	0,103		49,84
44	16		1,64
16 diff.	1,648	add for temp.	51,48

Correction for Pressure.

30 : 29,94 : : 51,48 : 51,37.

The quantity of oxygene at the mean would be 51,37 cubic inches.

100 : 55 : : 51,37 : 28,25.

Therefore 28,25 cubic inches of carbonic acid gas were produced.

100 : 47,26, : : 28,25 : 13,35 grains.

13,35 : 3,8 : : 100 : 28,46.

Then, according to this experiment, 100 grains of carbonic acid contain 28,46 of the carbonaceous part of the plumbago.

Calculation by Oxygene.

100 : 33,82 : : 28,25 : 9,55 grains of oxygene
consumed 3,80 plumbago.

13,35

Calculation by carbonic acid 13,35

First Experiment on animal Charcoal.

Thermometer 60° FAHRENHEIT, barometer 30,23.

Muscular fibre distilled in a coated glass retort left a black shining coal, 4 grains of which were put into the tray. Our oxygene left a residuum of 2 parts in 100. The tray and its contents being placed in the platina tube, was heated to redness for 8 minutes. The first time the gas was passed, a lambent flame filled the whole length of the glass tube, and the gas became turbid or milky. It was passed frequently through the heated tube, but we observed no repetition of the flashes. Hence we conjecture that if the diamond had contained hydrogene, we should probably have had a similar appearance. After the experiment all the apparatus was, as usual, perfectly tight, and the volume of gas unaltered. On examining the platina tray a minute portion of charcoal remained, and a quantity of saline matter adhered to it so firmly, that it became difficult to ascertain the quantity of carbon consumed, and we forbore to make the calculation; we however examined the gas.

Lime water absorbed	40	parts from 100
The tests for oxygene	54	
Residuum - -	6	or an increase of 4 per cent.
	100	

Second Experiment on animal Charcoal.

Thermometer 59° FAHRENHEIT, barometer 29,45.

Some of the animal charcoal of last experiment was heated to redness under sand for one hour. 4 grains were placed in the platina tray; and as we were so much embarrassed in the last experiment with the saline matter which adhered to the tray, we exactly balanced it with its contents. Our oxygene, made as usual, left a residuum of 2 parts in 100, and we began with 49,84 cubic inches. When every thing was adjusted, and the platina tube red hot, on passing the oxygene, flashes resembling lightning ran along the glass tube; and this was repeated 5 or 6 times. The whole of the gas became very cloudy, exhibiting a turbid milky appearance. The tube was rendered white hot by the combustion of the carbonaceous matter in oxygene. The fire was kept up about 8 minutes, and the gas passed several times. When all was cool, we could observe no alteration in the volume of gas by the register. The tray contained a mixture of salts; and being weighed, was lighter by 3,2 grains. This loss was not wholly carbon, for it is well known that animal substance contains a variety of salts, as phosphates, muriates, &c. some of which, though not volatile in a low red heat, might be decomposed and dissipated in the intense white heat produced by the combustion of the carbonaceous matter in oxygene; and we accordingly found the internal parts of the gasometers and tubes very slightly covered with a sort of efflorescence. On examining the gas after the experiment,

Lime water absorbed	41	parts from 100
The tests for oxygene	55	
Residuum - -	4	or an increase of 2.
	100	

Correction for Temperature.

60°	49,84	
59	,10	add for temp.
1 diff. or 0,103	49,94	

Correction for Pressure.

30 : 29,45 . : 49,94 : 49,02.

The quantity of oxygene at the mean would therefore be 49,02 cubic inches.

100 : 41 : : 49,02 : 20,09

The carbonic acid gas produced was therefore 20,09 cubic inches.

100 : 47,26 : : 20,09 : 9,49

and this carbonic acid weighed 9,49 grains.

Now the coal in the tray had lost 3,2 grains; but as the whole of this was not carbon, but part of it volatile saline matter, &c. we shall endeavour to estimate the carbon by the experiment on plumbago. When 13,35 grains of carbonic acid contained 3,80 grains of carbon,

13,35 : 3,80 : : 9,49 : 2,70.

The quantity of carbonic acid produced in this experiment, therefore, contained 2,70 grains of carbon.

Loss	3,20	
Carbon	2,70	
Leaves	,50	for volatile saline matter, &c.

So that this being granted, the present experiment agrees with the foregoing.

In two of our first experiments with box-wood charcoal, the calculations gave us in one case 29,75 parts of carbon in 100 of carbonic acid, and in the other 30,68; but we were not then fully aware of the absorption of water by charcoal, which rendered the quantity of real carbon employed less than indicated by the weight. Also in another experiment, in which 4 grains of diamond were consumed, the calculation gave us 29,96 per cent. of diamond in carbonic acid; but apprehending that a slight degree of inaccuracy had crept into this experiment, we have not detailed it with the rest; but we have thought it right to give a simple statement of matters of fact; in no one instance have we endeavoured to strain or accommodate these to suit any particular theory, being fully aware that every experiment, carefully made and faithfully recorded, will remain an immutable truth to the end of time, while hypotheses are constantly varying, and even the most beautiful theories are liable to change.

The experiments above related give us the following results.

	By carbonic Acid.	By Oxygene.
Box-wood charcoal	28,92	28,77
1st expt. diamond	28,95	28,81
2d expt. diamond	28,82	28,72
Stone coal -	28,20	28,27
Plumbago -	28,46	28,46
	5)143,35	5)143,03
mean	28,67	28,60

Hence we conclude that 100 grains of carbonic acid contains 28,60 of carbon, which does not greatly differ from the results of the experiments of SMITHSON TENNANT, Esq. on the nature of diamond. See Phil. Trans. 1797.

This gentleman made his experiment in the following

manner. A quarter of an ounce of nitrate of potash was rendered somewhat alkaline by exposure to heat, in order that it might more readily absorb carbonic acid; it was then put into a gold tube with $2\frac{1}{2}$ grains of diamond, and being subjected to heat, the diamond was converted into carbonic acid, by uniting with the oxygene contained in the nitric acid. The carbonic acid thus produced combined with the potash, and on pouring a solution of muriate of lime into a solution of this salt, he obtained a precipitate of carbonate of lime: this being decomposed by muriatic acid, gave as much carbonic acid gas as occupied the space of 10,1 ounces of water. The thermometer was at 55° FAHRENHEIT, the barometer 29,80. In a second experiment he procured a larger quantity, or equal to 10,3 ounces of water.

If we therefore consider an ounce of water as consisting of 480 grains, and a cubic inch of water equal to 253 grains, and then make the proper corrections for temperature and pressure, one of his experiments will give about 27 per cent. the other about 27,80 for the carbon in carbonic acid, which is somewhat less than our estimate; but the difference may easily be accounted for, from the different methods employed.

The experiments of GUYTON, as detailed in the *Annales de Chimie*, vol. XXXI, page 76, are liable to very strong objections; but at the same time the candid manner in which he has related every circumstance merits considerable praise. It is impossible, however, not to observe, that the quantity of gas before and after the experiment could not, from the construction of his apparatus, be very rigorously ascertained. We object also to nitrous gas as a test for oxygene; and as it is acknowledged that the wooden support included in the oxygene gas took fire, the product of carbonic acid must have

been influenced by it; so that if no chance of error had existed in estimating the carbonic acid gas from the residuum after barytic water had absorbed a part, still the result would not have been satisfactory.

The experiments which we have had the honour of laying before this Society prove several important points:

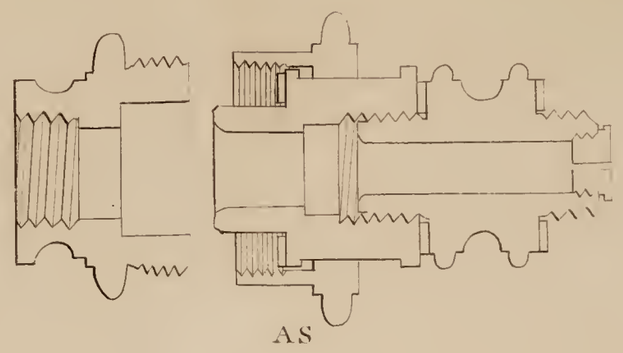
1st. That the estimate given by LAVOISIER, of 28 parts of carbon in every 100 parts of carbonic acid, is very nearly correct; the mean of our experiments makes it 28,60.

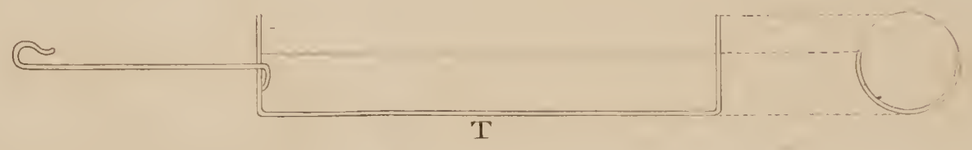
2dly. That the diamond is pure carbon; for had it contained any notable proportion of hydrogene, it must have been discovered, either by detonating with the oxygene, as in the case of animal charcoal, or by diminishing the quantity of oxygene gas.

3dly. That well burnt charcoal contains no sensible quantity of hydrogene; but if exposed to the air for a few hours it absorbs moisture, which renders the results uncertain.

4thly. That charcoal can no longer be considered as an oxide of carbon, because, *when properly prepared*, it requires quite as much oxygene for its combustion as the diamond. This is also the case with stone coal and plumbago.

5thly. It appears that diamond and all carbonaceous substances (as far as our present methods of analysis are capable of demonstrating their nature) differ principally from each other in the state of aggregation of their particles. BERTHOLLET has well remarked, that in proportion as this is stronger, decomposition is more difficult: and hence the variety of temperatures required for the combustion of different inflammable substances.





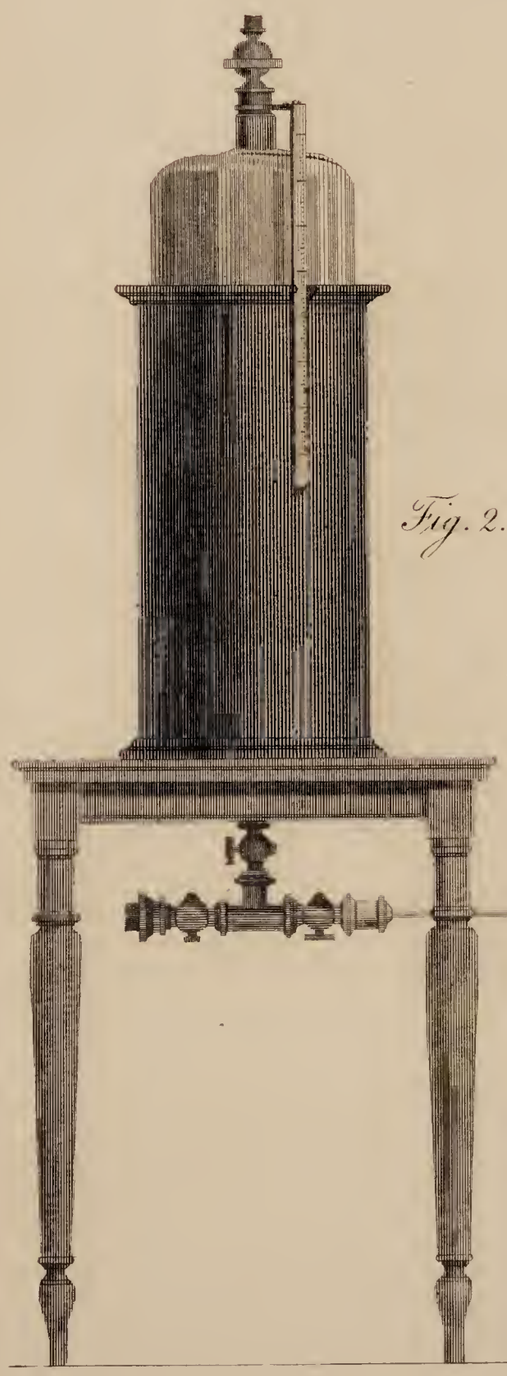


Fig. 2.

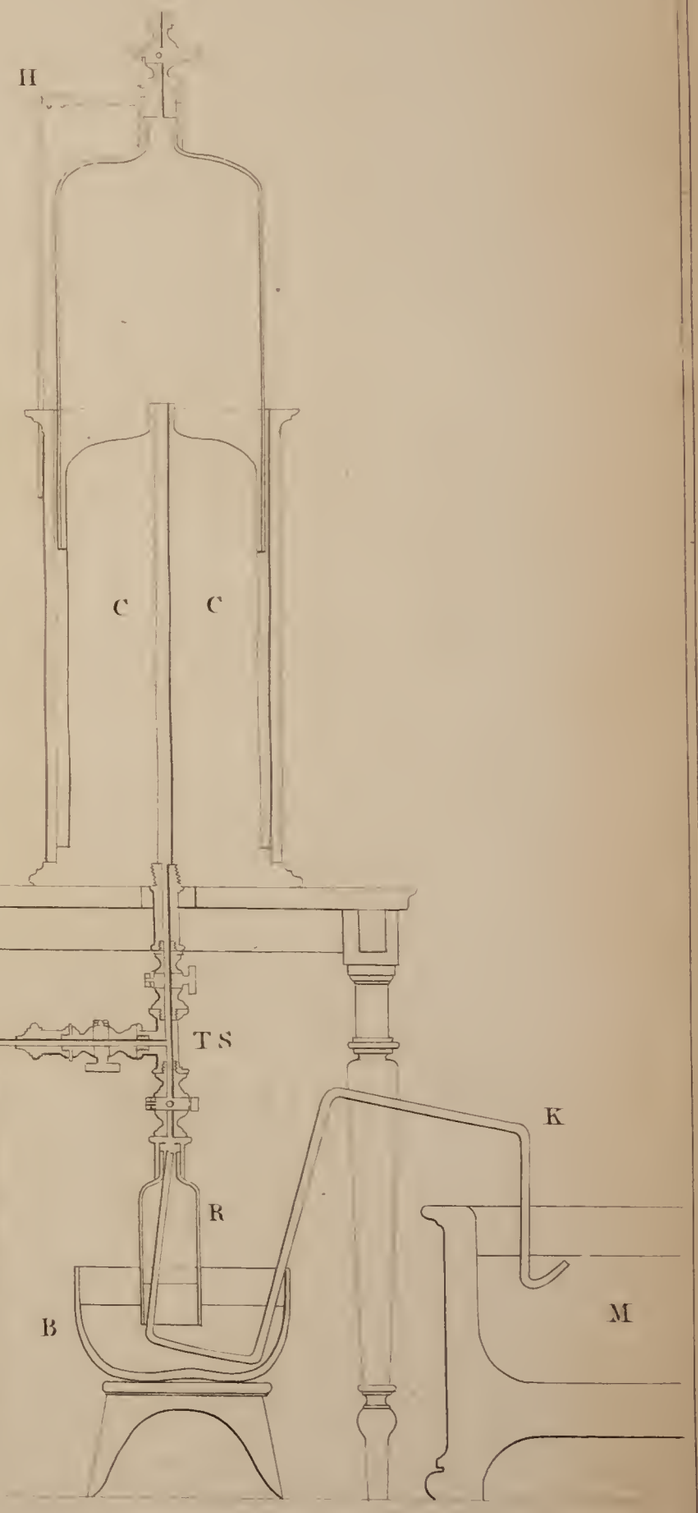


Fig. 1.

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XV. *An Account of the Relistian Tin Mine. By Mr.* Joseph Carne, *in a Letter to* Davies Giddy, *Esq. M.P. F.R.S.*

Read May 7, 1807.

DEAR SIR, *Penzance, April 22*, 1807.

WHEN I mentioned the occurrence of pebbles of chlorite shist, cemented by crystalized tin, in the Relistian mine, you expressed a wish to receive a particular account of this novel circumstance.

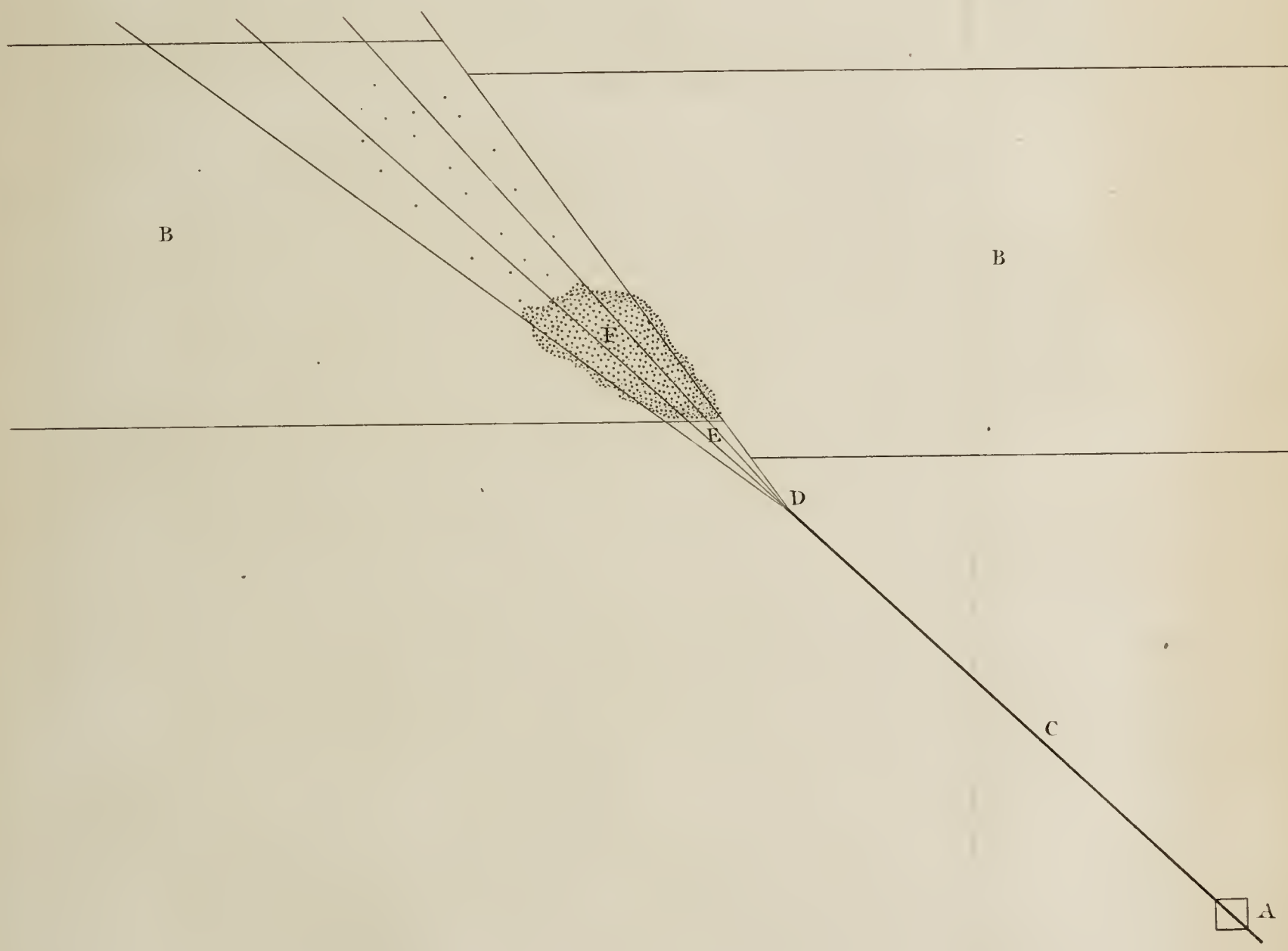
The Relistian mine is nearly on a level with the surrounding country. The lode has been seen at the depth of 12, 25, 50, 65, 75, 81, and 90 fathoms from the surface. It is of different width in different parts; the extreme width is 36 feet, and in this part it is principally worked. As it extends east and west (which is its due course), its width gradually diminishes, till at the distance of 100 fathoms east it is but 5 feet wide. It is composed (excepting the metallic substances) of shist, chlorite, and quartz. In some parts the shist predominates, and in others the chlorite; the quartz is throughout the smallest component part. The engine shaft (see plan (A)) is situated 8 fathoms north of the widest part of the lode (B). In sinking the shaft a flookan (C), about 2 inches wide, was dicovered, bearing a south-east course, which cut the lode at an angle of 45°; and heaved and disordered it.

At the depth of 12, 25, and 50 fathoms, nothing was discovered in the lode but the cavities from which the ore had been taken away during the former period of working the mine.

At 65 fathoms in depth were found, close to the flookan, a great number of angular fragments of shist, cemented by the same substance.

At the depth of 75 fathoms the flookan (C) became 4 inches wide in the shaft (A), and continued of that size for 10 fathoms; it then became divided into 4 parts or branches (D), each diverging from its former course, and in this state it continued through the lode (B), of which the first 3 feet were composed of copper pyrites (E), and then was discovered a body of pebbles (F), nearly 12 feet square, extending in width to the extreme branches of the flookan. In this part of the lode the shist greatly predominates; of course the pebbles are generally composed of shist, cemented in some parts by the same substance or chlorite, in others by oxyde of tin, which is generally crystallized, and in some of the crevices there is a little copper pyrites. It is singular that a few pebbles (perhaps not more than half a score) were found of quite a different nature from the others; they were composed of tin in quartz coated with chlorite.

The pebbles did not continue in a body to the height of more than 2 fathoms; but scattered bunches, and single pebbles, were found 4 fathoms above and 6 fathoms below the place in which they were at first discovered. It is only necessary to add, that the lode has since been worked 15 fathoms deeper than where the pebbles occurred; it there consists for



Basire Sc.

the most part of chlorite formed in a regular manner; not the least trace of pebbles is to be seen, nor indeed of any disturbance in the strata.

I am, dear Sir,

very sincerely yours,

JOSEPH CARNE.

Penzance, Cornwall.

XVI. *An Analysis of the Waters of the Dead Sea and the River Jordan. By* Alexander Marcet, *M.D. one of the Physicians to Guy's Hospital. Communicated by* Smithson Tennant, *Esq. F.R.S.*

Read June 18, 1807.

THE Dead Sea, or Lake Asphaltite, is situated in the southern part of Syria, near Jerusalem, and occupies an extent of about 60 or 70 miles in length, and from 10 to 20 in breadth. This lake has been from time immemorial celebrated on account of the intense saltness of its waters, which is such as to prevent either animals or vegetables from living in it, a peculiarity from which it has derived its name. It appears that this saline quality has existed in the earliest ages; for independently of the frequent allusions made to it in the Scriptures, we find it described by several ancient authors, amongst others by STRABO,* who wrote during the reign of AUGUSTUS, by TACITUS,† and by PLINY.‡ Amongst modern travellers, POCOCKE,§ VOLNEY,|| and others, have noticed and described this singular spot.

But although the most obvious peculiarities have for a long time been in some degree known, the only chemical analysis I have been able to find on record is that which was published

* STRABONIS *Geogr.* vol. ii. p. 1107.
† TACITUS, lib. v. *Hist.* cap. vi.
‡ PLINII lib. v. cap. xv. and xvi.
§ POCOCKE's *Travels* in 1743, ii. p. 34.
|| VOLNEY, i. 281.

in the "*Memoires de l'Academie des Sciences*" for the year 1778, by Messrs. MACQUER, LAVOISIER, and SAGE. The names of LAVOISIER, and of his two distinguished associates, might appear to render any further investigation of the nature of this water superfluous; but whoever has perused the paper in question, must be convinced, that these gentlemen, however correct in their general statements, neither attained that degree of accuracy of which modern analysis is susceptible, nor did they bestow on the subject that share of attention which is indispensable in minute analytical experiments.

The gentleman to whom I am indebted for the specimen of the water of the Dead Sea which is the subject of this Paper, is Mr. GORDON of Clunie, who recently travelled in that country and undertook, not without some difficulty and danger, an excursion from Jerusalem to this remarkable lake. There he himself filled and brought to Sir JOSEPH BANKS a phial containing about one ounce and a half of this water, carefully corked, and in a state of perfect preservation. The same gentleman brought also in another phial, somewhat larger, a specimen of the River Jordan, which runs into the Dead Sea, without having any outlet, so that the river might be expected to hold in solution ingredients analogous to those of the Lake itself. These specimens Sir JOSEPH put into the hands of Mr. TENNANT, for examination. But knowing that I was engaged in similar researches, Mr. TENNANT was so obliging as to entrust me with this analysis, and to afford me frequent opportunities of availing myself of his assistance in the course of the enquiry.

Being possessed but of a small quantity of this water, a further supply of which could not easily be procured, I was

anxious not to waste any considerable portion of the specimen by preliminary trials. With this view, I began by making a variety of comparative experiments on artificial solutions, in order to ascertain the accuracy of different modes of operating; and knowing by LAVOISIER'S analysis, and also by the general effects of reagents applied to minute quantities of the water, what were the principal ingredients which I might expect to find in it, I made solutions, the contents of which I had previously ascertained with precision, so that by analysing these solutions in different ways, I had an opportunity of judging of the degree of accuracy that could be expected from a variety of methods. Some of these trials I shall briefly relate; for although not strictly belonging to the particular analysis in question, yet I conceive that they may be of some general use, in pointing out the most eligible method to be pursued in inquiries of this kind. Indeed it must be confessed that the minute chemical examination of any individual substance, requires so much time and patience, that to obtain a knowledge of that substance only, would seldom appear a sufficient inducement to such a laborious undertaking, was it not always more or less connected with other useful collateral objects.

§ I.

General Properties of the Dead Sea.

1. One of the most obvious peculiarities of the Dead Sea-water, is its specific gravity, which I found to be 1,211, a degree of density scarcely to be met with, I believe, in any other natural water. The circumstance of this lake allowing bodies of considerable weight to float upon its surface, was

noticed by some of the most ancient writers. STRABO, amongst others, states that men could not dive in this water, and in going into it, would not sink lower than the navel; and POCOCKE, who bathed in it, relates that he could lie on its surface, motionless, and in any attitude, without danger of sinking. These peculiarities, which I, at first, suspected of being exaggerated, are fully confirmed by Mr. GORDON, who also bathed in the lake, and experienced all the effects just related.

2. The water of the Dead Sea is perfectly transparent, and does not deposite any crystals on standing in close vessels.

3. Its taste is peculiarly bitter, saline, and pungent.

4. Solutions of silver produce from it a very copious precipitate, showing the presence of marine acid.

5. Oxalic acid instantly discovers lime in the water.

6. The lime being separated, both caustic and carbonated alkalies readily throw down a magnesian precipitate.

7. Solutions of barytes produce a cloud, showing the existence of sulphuric acid.

8. No alumine can be discovered in the water by the delicate test of succinic acid combined with ammonia.

9. A small quantity of pulverised sea salt being added to a few drops of the water, cold and undiluted, the salt was readily dissolved with the assistance of gentle trituration, showing that the Dead Sea is not saturated with common salt.

10. None of the coloured infusions commonly used to ascertain the prevalence of an acid or an alkali, such as litmus, violet, and turmeric, were in the least altered by the water.

§. II.

Preliminary Experiments to ascertain the Composition of the Salts concerned in this Analysis.

Having satisfied myself by these preliminary experiments that the Dead Sea contained muriat of lime, muriat of magnesia, and selenite, and having no doubt both from the taste of the water, and from LAVOISIER's statement,* that it contained also common salt, I proceeded to the comparative experiments above mentioned.

The first indispensable step was to ascertain with accuracy the proportions of acid, and base, in the three muriats just named. This I had already done in the course of a more general inquiry which I began some time ago in conjunction with Mr. TENNANT, and which has been of great use to me on the present occasion. But as the particulars of that series of experiments may probably be published at some future period, I shall now confine myself to such general statements as immediately belong to my subject.

1. The composition of muriat of lime was ascertained by pouring a known measure of muriatic acid on a piece of pure marble of known weight, and more than sufficient to saturate the acid. The remaining portion of marble being then weighed, and the solution evaporated and heated to redness, the proportions of acid and earth were easily deduced. But in order to draw such an inference, it was necessary to ascertain with precision the quantity of pure lime in a given weight of marble,

* MACQUER, LAVOISIER, and SAGE, discovered the three muriats, but overlooked the small quantity of selenite.

which, from a number of experiments performed with great care by Mr. TENNANT and myself, appeared to be 56,1 parts of lime in 100 of marble. From a great variety of trials, made with considerable attention, and with due allowance for any accidental circumstances, muriat of lime appeared to consist of 50,77 parts of lime, to 49,23 of muriatic acid.

2. To ascertain the proportions of earth and acid in muriat of magnesia, required a synthetic process somewhat different. To a known weight of pure magnesia perfectly calcined, a known quantity of acid* was added, and after the whole of the magnesia was dissolved, the remaining portion of acid was saturated by marble. From the loss sustained by the marble, and the known proportions of acid and magnesia used, the composition of muriat of magnesia (supposed perfectly free from water) was deduced, and the proportions resulting from several careful trials, were 43,99 parts of magnesia, to 56,01 of muriatic acid.

3. Muriat of soda was analysed by various methods. But the only one which I shall now relate consisted in precipitating the acid by a solution of silver from a known weight of muriat of soda, and inferring the proportion of acid and alkali from the quantity of luna cornea obtained. This however required a previous exact knowledge of the proportions of acid and silver in luna cornea. In order to ascertain this point, a known quantity of acid was precipitated by nitrat of silver, and the weight

* By a *known quantity* of acid, is meant as much acid as will dissolve a known weight of marble. In all these experiments the quantities of acid were not weighed, but measured by means of a peculiar apparatus, and the real weights or intrinsic quantities of acid, corresponding to the measures in question, were easily deduced from the results above mentioned.

of the luna cornea, after being melted and heated to redness, indicated 19,05 parts of acid to 80,95 of oxyd of silver. The composition of common salt, calculated from these data, proved to be 46 parts of acid to 54 of soda.

§ III.

Comparative Analyses of artificial Solutions.

I shall not enter into all the particulars of the various analyses of artificial solutions, resembling the water of the Dead Sea, which directed me in the choice of the method which I ultimately adopted. But it may be proper to state, in a summary manner, the principal means which were tried, and their respective defects and advantages.

These artificial mixtures all contained the three muriats above mentioned, but in each of them the small quantity of selenite was altogether disregarded.

1. The first of these solutions was evaporated to dryness, and the residue exposed for near an hour to a red heat in a platina crucible pretty closely covered. The object of this was to drive off the acid from the magnesia (muriat of magnesia being decomposable by heat), and after separating this earth from the other salts by means of distilled water, to precipitate the lime by carbonat of ammonia, and to obtain the muriat of soda by evaporation to dryness. But I soon found that the complete decomposition of muriat of magnesia by heat, under these circumstances, was extremely difficult, if not impossible, and accordingly the results obtained from this method indicated considerably less magnesia and proportionally more lime,

than the solution really contained. The quantity of common salt was tolerably accurate.

2. From another similar solution the lime was precipitated by oxalat of ammonia; the magnesia was separated by heat in an *open* crucible, and the common salt was obtained, as before, by evaporation and exposure to a low red heat. The result was satisfactory both as to the lime and magnesia; but as the separation of the latter could only be completed by long continued heat, in an open vessel, I found the muriat of soda materially reduced by sublimation, and was therefore obliged to abandon this mode of proceeding.

3. From a third artificial solution, the lime was precipitated by oxalat of ammonia, the magnesia by carbonat of ammonia recently prepared, and the sea salt was obtained as usual by evaporation and desiccation in a low red heat. The object of this mode of operating was to supersede the necessity of applying a red heat in the first instance. But I was again disappointed; for the magnesia was but imperfectly precipitated; and in order to separate the last portions of this earth, it was necessary to calcine the last residue containing the muriat of soda, which gave rise to the same objections as in the former experiments.

4. The last and most successful method consisted in dividing the artificial solution into two portions. From one of these the muriatic acid was precipitated by nitrat of silver, and its quantity ascertained. From the other the lime was separated by oxalat of ammonia, and the magnesia by caustic potash;*

* Or by carbonat of ammonia. In this case the precipitation of magnesia is not so perfect; but the precipitate falls down more quickly, and the separation of any remaining portion of this earth may be ultimately completed by heat.

and the respective portions of acid belonging to each of these earths being calculated, the quantity of muriat of soda was inferred from the remaining quantity of acid.

This method afforded remarkably accurate results. The only objection to it seems to be that the muriat of soda being only estimated, and not actually obtained, if any error be made either in the estimation of the acid or in the separation of the lime and magnesia, these errors must also ultimately affect the computation of the muriat of soda, without allowing any immediate means of detecting them. This objection, however, is in a great degree removed, by a comparison of the two portions of the solution, from one of which the common salt can be obtained undecomposed; and the present method has this additional advantage, that the quantity of acid is a sort of check, which, when connected with some other point of comparison, prevents any gross error in the computation of the earths, from escaping notice.

This plan being very similar to that which I actually followed in the analysis of the water of the Dead Sea, it may be worth while to mention the summary results of the comparative experiments which decided me in its favour.

The artificial solution contained:

	Salts.		Acid.
Muriat of lime -	8,17 grains		4,02 grains.
Muriat of magnesia	26,10	=	14,62
Muriat of soda -	25,00	=	11,50
	59,27*	=	30,14

* These happened to be very nearly the real proportions of salts in the Dead Sea; yet this coincidence was a matter of mere accident; for when I mixed up the ingredients, I was led to suppose from LAVOISIER'S paper, that their proportion in the Dead Sea was very different from that which I afterwards ascertained.

And the contents inferred by the foregoing method were:

	Salts.		Acid.
Muriat of lime -	8,14	=	4,01 grains.
Muriat of magnesia	25,62	=	14,35
Muriat of soda -	25,47	=	11,72
	59,23	=	30,08

§ IV.

Analysis of the Dead-Sea Water.

I now come to the actual examination of the water of the Dead Sea, the particulars of which will be found much shortened by the preceding observations.

1. 20 grains of this water (the whole supply of which amounted only to 540 grains) were put in a glass capsule, and slowly evaporated in a water bath, by means of an appropriate apparatus, the temperature of the capsule being constantly kept within 5 degrees of 180°. The object of this experiment was simply to know the weight of the solid contents of the water, dried under various degrees of heat, and to observe the appearances produced by evaporation. After a few hours, and when the residue had ceased to lose weight, the saline mass, whilst still warm, appeared in the form of a white semitransparent incrustation, which yielded to the touch, being soft, and of a pulpy consistence. In cooling it became hard, and of a much more opaque white colour. When examined with attention, the borders of this mass were found covered with small cubic crystals, and the same appearance was observed, though less conspicuously, in the centre under the saline incrustation, when in the state of semifusion just described. On standing in the

air for some time, the white opaque mass gradually absorbed water from the atmosphere, and returned to a liquid state. The 20 grains of the water, thus evaporated and dried at 180°, weighed, whilst still warm, 8,2 grains.

2. The same saline mass being afterwards exposed in a sand bath to the temperature of 212° FAHRENHEIT, was reduced to 7,7 grains. Hitherto not the least smell of muriatic acid was perceived, nor did any decomposition appear to take place.

3. But having raised the heat about 15° higher, the residue, after a few minutes, was found reduced to 7,4 grains; and on redissolving it, a few insoluble white particles appeared floating in the solution, showing an incipient decomposition in the muriat of magnesia.

It appears from these experiments that 100 parts of the Dead Sea water yield 41 of salts dried at 180°, and 38,5 dried at 212°.* What proportion these quantities bear to the same salts, when perfectly deprived of water, will be seen from the subsequent results. I now pass on to the chemical examination of the water.

4. To 100 grains of the Dead Sea water a few drops of muriat of barytes being added, a precipitate was obtained, which, after being well washed and exposed to a low red heat on a piece of laminated platina, weighed 0,09 grain, which, allowing for the unavoidable loss attending the manipulation of such very minute quantities, may safely be called 0,1 grain.

* If the quantity of materials upon which these results are founded, should appear too small, I would observe that if the bulk of salt be considerable, it is impossible to dry it accurately, owing to the crust which forms on the surface, and prevents the escape of moisture. But at any rate no perfect accuracy can be relied on respecting this kind of limited desiccation, as its completion depends in a great degree on the shape of the vessel, the thickness of the stratum of salt, &c.

This residue, on being heated with fluat of lime, instantly ran into a globule, and was evidently sulphat of barytes.

5. To another portion of the Dead Sea water, weighing 250 grains, a solution of nitrat of silver being added till it ceased to produce any precipitate, a quantity of luna cornea was obtained, which after careful edulcoration and exposure to a red heat, weighed 163,2 grains, a quantity equivalent, according to the proportions above stated (§ II. 3), to 31,09 grains of real acid.

6. To the remaining solution a little muriat of ammonia was added, in order to remove the unavoidable small excess of silver, and this new precipitate was separated and well edulcorated.

7. The clear fluid, which had been much increased in bulk by these edulcorations, being concentrated to about 3 ounces, a strong solution of oxalat of ammonia, warm, but not nearly boiling,* was added to it, by which a precipitate was obtained, which collected and washed with the usual precautions, and after deducing 0,076 grains of lime † for the 0,136 grains of selenite belonging to 250 grains of the water, yielded 4,814 grains of pure lime = 4,66 grains acid = 9,48 grains muriat of lime.

I should not omit mentioning that the method which I used in all my experiments to ascertain the quantity of pure lime in

* The precipitates of lime by oxalat of ammonia subside more readily if the solutions be used warm; but when concentrated and heated to the boiling point, this test acts also in some degree on magnesia, a circumstance which in the present instance, was to be particularly avoided.

† The proportion of lime in selenite, and of acid in sulphat of barytes, are taken from a paper of Mr. Chenevix, in Nicholson's Journal, Vol. II. in which they are stated to be 56,4 of lime in 100 parts of selenite, and 24 parts of acid in 100 parts of sulphat of barytes.

oxalat of lime, consisted in driving off the oxalic acid by a low-red heat, and adding to the calcareous residue, then converted into a subcarbonat, a known quantity of muriatic acid more than sufficient to dissolve the whole lime. A piece of marble of known weight was afterwards added to take up the excess of acid, and from these data the quantity of lime was calculated with great precision.

8. The clear solution containing nitrat of magnesia, nitrat of soda, and a small excess both of oxalat and muriat of ammonia, and amounting in bulk to about 4 ounces, was exposed to the heat of a lamp for concentration; but in a few minutes the mixture became turbid and began to deposite a white powder, which, from former observations, I supposed to be oxalat of magnesia. To this solution concentrated to between 2 and 3 ounces, and still warm, I added carbonat of ammonia with excess of pure ammonia. A considerable precipitation immediately appeared, and the mixture became opaque and milky. The next morning, however, the fluid had become quite transparent, and instead of a white impalpable precipitate, I found clusters of perfectly pellucid crystals spread over the bottom of the vessel, with distinct interstices between them.

This salt was no doubt an ammoniaco-magnesian carbonate; and the remaining solution, although still containing, as will presently appear, a vestige of magnesia, was so far free from it, as not to have its transparency disturbed by caustic potash. These crystals, after being well washed in distilled water, were exposed to a gentle heat to drive off the ammonia, in consequence of which they crumbled down into a white impalpable powder, exactly resembling common carbonat of magnesia. This powder being then treated, and its quantity estimated, in

a way similar to that which had been employed with the lime; and being increased by the addition of about 0,5 grains of a similar precipitate (which had escaped the action of the carbonat of ammonia and was obtained from the last remaining solution by evaporation and calcination), amounted to 11,10 grains of pure magnesia = 14,15 grains of muriaic acid = 25,25 grains of muriat of magnesia.

9. The muriat of soda was next estimated from the 12,28 grains of muriatic acid found to remain after subtracting the sum of the two portions (4,66 grains and 14,15 grains) belonging to the lime and magnesia, from the 31,09 grains, or sum total of acid. These 12,28 grains gave according to the proportions before mentioned (§ II. 3), 26, 69 grains of muriat of soda

10. From these several results brought into one view, and the salts being supposed heated to redness, 250 grains of the Dead-Sea water appear to contain,

	Salts.		Acid.
Muriat of lime -	9,480 grains		4,66 grains.
Muriat of magnesia	25,25	=	14,15
Muriat of soda -	26,695	=	12,28
Sulphat of lime -	0,136	-	-
	61,561		31,09

And therefore 100 grains of the same water would contain,

	Grains.
Muriat of lime - -	3,792
Muriat of magnesia -	10,100
Muriat of soda - -	10,676
Sulphat of lime - -	0,054
	24,622

§ V.

Second Analysis of the Dead Sea Water by a Method somewhat different from the former.

In the mode of proceeding just related, some small loss in the earths might naturally be suspected to have taken place in consequence of the previous separation of the acid and indispensable edulcorations. Besides, the muriat of soda being necessarily decomposed by the first part of the process, the analysis could not have been considered as quite satisfactory, had not the common salt been procured unaltered by some other process.

1. In order to obtain these points, 150 grains of the water were treated, with regard to the lime and magnesia, exactly as in the former analysis; but in this case, the acid, instead of being actually separated by silver, was only calculated from the former estimation (§ IV. 5).

2. The result proved perfectly agreeable to my expectation. It yielded a little more lime and magnesia than the former analysis, but this excess was scarcely perceptible. With regard to the muriat of soda, I was able actually to procure by evaporation, as much as 13,1 grains of this salt, the actual quantity of which, inferred as in the preceding analysis, was 15,54 grains, a difference easily accounted for by the necessity of heating the salt to redness for its ultimate separation.

3. On summing up the contents of these 150 grains of the water, they appeared to be as follow:

	Salts.		Acid.
Muriat of lime -	5,88 grains		2,89 grains.
Muriat of magnesia	15,37	=	8,61
Muriat of soda -	15,54	=	7,15
Selenite - -	0,08	-	- -
	36,87		18,65

And consequently the proportions of these salts in 100 grains of the water would be:

	Grains.
Muriat of lime - - -	3,920
Muriat of magnesia - -	10,246
Muriat of soda - - -	10,360
Sulphat of lime - - -	0,054
	24,580

The coincidence of these results with those of the former analysis was such as I could scarcely have expected to increase by further trials. The last statement, however, I consider as the most accurate of the two.

It may therefore be stated in general terms, that the Dead-Sea water contains about one fourth of its weight of salts supposed in a state of perfect desiccation; whilst, as I observed before, if these salts be only desiccated at the temperature of 180°, they will amount to 41 per cent. of the water. This great difference between the two states of desiccation depends on the great affinity which muriats, particularly that of magnesia, have for water. Muriat of soda is scarcely at all concerned in this difference: for I found, not without surprize, that 100 grains of artifical cubic crystals of muriat of soda, being fused

and heated to redness in a platina crucible, lost at most half a grain.

In the analysis of MACQUER and LAVOISIER, the solid contents of the Dead Sea are estimated at about 45 per cent. of the water, and in the proportions of nearly 1 part of common salt to 4 of muriat of magnesia, and 3 of muriat of lime; proportions widely different from those which I had obtained. But their mode of operating, which they candidly relate, was so evidently inaccurate with regard to the separation and desiccation of the salts, and in general so deficient in the estimation of quantities and proportions, that these eminent chemists cannot be considered as having aimed, in this instance, at any thing like an exact analysis.

It may be observed also, that these gentlemen found the specific gravity of the water 1,240 instead of 1,211, as I have stated it to be; but it appears that their specimen had suffered some evaporation previous to their experiments, since they found crystals of common salt in one of their bottles, which could not have happened without evaporation. Besides, the specimen which I examined was, I understand, brought from a part of the lake not more than two miles distant from the mouth of the Jordan, a circumstance which may perhaps account for its being somewhat more diluted, than it might be found in other parts.

§ VI.

Analysis of the Water of the River Jordan.

As I had scarcely two ounces of this water, and as it contained but a very small proportion of saline ingredients, it would have been in vain to aim at analyzing it with strict

accuracy. Yet I thought it worth while to endeavour to form as exact an estimation of its contents as I could, on account of its connection with the Dead Sea, into which, as was observed before, it pours its waters, and appears to remain in a stagnating state. This specimen was brought from a spot about three miles distant from that where the river enters the Dead Sea.

From the perfect pellucidity of this water, its softness, and the absence of any obvious saline taste, I was led to suppose that it was uncommonly pure, and could in no degree partake of the peculiar saline qualities of the Dead Sea. But I was soon induced to alter my opinion by the following results.

1. The same chemical reagents, as were used to ascertain the general properties of the Dead Sea water, being applied to this, produced analogous effects. The same three muriats and even the vestige of selenite, were distinctly discovered; and this resemblance became more striking in proportion as the water was concentrated by evaporation.

2. 500 grains of this water being evaporated at about 200°, the dry residue weighed exactly 0,8 grains. This makes the solid ingredients amount only to 1,6 grains in 1000 grains of the water, a singular contrast with the Dead Sea, which contains nearly 300 times that proportion of saline matter. As the water was concentrating, a few white particles were perceived on its surface, and a few others gradually subsided. When dried, the residue appeared in the form of a white incrustation, the upper edge of which exhibited great numbers of very minute crystals, which from their saline taste, and their cubic shape discoverable by the aid of a microscope, were evidently common salt.

3. Distilled water being thrown on this residue, a minute portion of it remained undissolved, and on pouring an acid on this substance, a distinct effervescence was produced, showing the presence of carbonat of lime.

4. From the clear fluid a precipitate was obtained by oxalat of ammonia, which, dried but not calcined, weighed 0,12 grains.

5. From the remaining clear solution a magnesian precipitate was produced by ammonia and phosphoric acid, which, after driving off the ammonia by heat, weighed 0,18 grains.

6. The solution had suffered too many alterations to allow me to separate, with any degree of accuracy, the muriat of soda; but from a variety of circumstances, I thought it not unlikely that it would have been found pretty nearly in the same proportions, with respect to the other salts, as it exists in the Dead Sea.

The inference I drew from this was, that the River Jordan might possibly be the source of the saline ingredients of the Dead Sea, or at least that the same source of impregnation might be common to both. This inquiry, however, would require a much more correct knowledge both of the proportions of the salts, and of local circumstances, than I have ben able to obtain.

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